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ART. I.—*Alexander von Humboldt, his early Life, his Education, his Writings, and his Books;* by HENRY STEVENS of Vermont, F.S.A. etc., 4 Trafalgar Square, London.

THE higher the sun the shorter the shadow; even so the greater the eminence of a philosopher the briefer need be his eulogy by the unphilosophical. Nevertheless so exceptional was Humboldt in himself, his early training, his achievements, it may not be deemed out of place here, though perhaps at the expense of wearying the unlearned who need not know so much, or worrying the learned who know it already, to recapitulate a few of the well known points in the life, education, and character of this illustrious man.

If the indifferent reader will run his quick eye over the titles of the seventeen thousand volumes recorded in the Catalogue of his Library he will, no doubt, see at a glance that in very many respects, it is the most extraordinary collection of modern

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[The above article has been contributed to the Journal at our request by Mr. Henry Stevens, a gentleman of well known bibliographical attainments, who purchased the library of Humboldt, not long after his decease, and who has arrived at many interesting conclusions in respect to the distinguished scientist, based chiefly upon the examination of his literary and scientific collections. It was originally written as a preface to a Catalogue of the Library prepared under the direction of Mr. Stevens. Owing to a change in Mr. Stevens's plan respecting its publication, some slight alterations have been made in it, but not in any way to affect the comments upon Humboldt.—EDS.]

scientific books, especially those pertaining to the physical sciences, ever brought together. It is true, many of the common every-day standard works of this class such as 'no library should be without,' are wanting here, particularly the long sets of scientific periodicals, the transactions of learned societies and other equally valuable and bulky books; but when it is remembered that the great philosopher, while in Paris, where for twenty years he was revising and finishing what he had done, and afterwards while at Berlin, where in his old age, in working up his masterpieces of thought, he brought back to the anvil that which he had hammered, always had at his elbow some of the best public and private libraries in France and Germany, as free to him at all times as his own, their absence from his own collection is accounted for. But the reader will observe that an unusually large proportion of Humboldt's books cannot be found in any other single library, public or private, in Europe or America, not excepting even the British Museum, the Imperial Library of Paris, the Royal Library of Berlin, or the Royal Society of London. Indeed it is a matter of fact, that on a careful examination in 1862–3, it was found that the library of the British Museum, was deficient in at least one-third of the titles entered in this catalogue, every one of which was deemed a desirable acquisition. These were mostly privately printed or unusual works. Almost every page of the catalogue contains books important in themselves, which no money can buy in the usual channels of commerce, but which in every tongue were drawn together from all parts of the world, by the irresistible gravitation of Humboldt's name.

About the year 1855 in this country an unfortunate public impression was achieved, for a purpose, and sedulously enforced by high authority, to the effect that the collection of large libraries of scientific and other books was not necessary to the *increase and diffusion of knowledge among men.* "There are two individuals," wrote a distinguished savant to the Committee of Congress, "who may, without qualification, be considered the most prominent scientific men of the nineteenth century—Cuvier and Humboldt. By what means have they given such powerful impulse to science? How have they succeeded not only in increasing the amount of knowledge of their age, but also in founding new branches of science? It is by their own publications and by aiding in the publications of others; by making large collections of specimens and other scientific apparatus, and not by the accumulation of large libraries. Humboldt never owned a book, *not even a copy of his own works*, as I know from his own lips. 'He was too poor,' he once said to me, 'to secure a copy of them'; and all the works he receives constantly from his scientific friends are distributed by him to

needy students." The very historical truth was then and is now that the two best scientific private libraries in Europe belonged to Cuvier and Humboldt, the one lining the splendid salons in which the baron held those historic reunions; the other described in this catalogue which contains all the books Humboldt ever wrote, many of them thumbed, worn and annotated.

Humboldt was a fortunate child of nature. A lucky star seems to have presided at his birth. On the third of June, 1769, occurred the rarest and most important phenomenon within the whole range of mathematics and astronomy; an event more favorable of its kind than any that had been observed since the Christian era began, or would again occur for hundreds of years; an event which Rittenhouse in America, Euler in Germany and many other eminent astronomers of other countries were deputed to observe for the benefit of mankind. The last *Transit of Venus*,* the great opportunity for determining the sun's parallax, and consequently the dimensions of our planetary system and our globe, was a conjunction of planets coincident with the birth of twelve imperial men of nature, more renowned than the twelve Cæsars. No other single year probably, before or since, ever produced such men as Napoleon, Wellington, Soult and Ney; Brunel, Mehemet Aly, Turner, Sir Thomas Lawrence, Châteaubriand, and Castlereagh; CUVIER and HUMBOLDT; men who upturned the world and set it right again; who revolutionized science, art, politics, states, and the affairs of mankind. Humboldt, in many respects the most gifted of them all, outlived them all.

With his life ended, and became historic, the first grand period of modern science. The times that developed the French Revolution ripened Humboldt, and made conspicuous others, many of them his friends and fellow laborers, among

* *Transits of Venus* are as rare as they are important. They occur in couples, in June and December, about eight years apart, and then not again for several generations. Kepler was aware of the phenomenon and as early as 1604 announced that one would take place in 1761, but young Horrocks of Liverpool with better tables, and additional data, calculated that there would be a *transit* on the 4th of Dec., 1639. He let a friend into his secret, and they two on the day named, for Venus was punctual, were the first ever known to observe it. It was soon calculated that one must have taken place on the 6th of Dec., 1631, and another in June, 1526, and that the next would not occur till the 5th of June, 1761. But of all the transits, past and to come, the climax would be, that of the 3d of June, 1769, when Venus passed across the sun's disc very near the center. The next one, but not visible in this country, will take place five years hence on the 8th of Dec., 1874, which will be a grand one for science, considering the great advance in scientific instruments, but far inferior to the last. If, however, it produces only half a dozen Cæsars it will be a godsend to this rapid century. *Let young folks take note of the date* 1874. Another will occur on the 6th of Dec., 1882, but not again till nearly five quarters of a century later, on the 7th of June, 2004; to be followed eight years after, on the 5th day of June, 2012; to be repeated in Dec., 2117, and so on.

whom may be named LaPlace, Lalande, Gay-Lussac, Kunth, Bonpland, Oltmans, Oersted, Bichat, Delambre, Bessel Berzelius, Davy, Robert Brown, Dalton, Herschel, DeCandolle, Latreille, Valenciennes, Audubon, David d'Angier, Arago, Gauss, Ritter, Müller, Leopold von Buch, Varnhagen von Ense, Compte, Biot, *et al.*, names themselves suggesting discoveries, inventions and unbounded knowledge.

ALEXANDER VON HUMBOLDT was born in Berlin on the 14th of September, 1769, and died there on the 6th of May, 1859, in the ninetieth year of his age. A rapid sketch of his youth and early manhood will serve to show how well he prepared himself, single-handed, to accomplish so much; more even than most of the learned academies of which he was a member, and certainly not less than any of the great Voyages of Discovery, and celebrated Exploring Expeditions conducted at the public expense. The course pursued with him was so peculiar, and contrasts so completely with the usual course of training in the colleges of this country, even in the scientific departments, that no pains have been spared to render this account as complete and exact as possible. Unroll the scrolls of biography and no name will be found a brighter example to stimulate, encourage and direct the youthful student than that of Humboldt.

He was fortunate in his father, Major von Humboldt, who gave him wealth and position; favored in his elder brother William, who gave him the best of companionship, whom he and the world alike loved and honored through a long life; and blessed in his noble mother, to whose virtues, devotion to her boys, energy of purpose, and common sense he owed a right start both in the political world, which he disliked, and in the physical world, which he adored. The earliest tutor chosen by the mother to teach and play with the two boys was Campe the educationist, who among other children's books, edited in German, Robinson Crusoe, a work which no doubt had its early influence in bending the youthful twig. When Alexander was eight years old his father died. The mother and her two boys then lived at Tegel the old homestead a few miles from Berlin. Alexander's constitution was not very strong. In the year 1779 the widow was fortunate enough to secure for their companion and tutor, G. J. Christian Kunth, a youth of twenty, a rare and noble fellow, in whom the mother could and did place implicit confidence. He took charge of the brothers, and became their guide, companion and friend, at the same time being in constant consultation with their mother. How admirably he guided them in their boyish games and studies, and how faithfully he labored for and with them in after life, both the brothers bear ample testimony. The next year, when Alexander was

eleven, he heard at Tegel private lectures on botany by Heim. Two years later Kunth used to take the boys to Berlin to pursue their studies with private masters. They studied together, but each was allowed to follow his own bent. Though Alexander differed widely from William in his inclination for some studies, yet both were alike ardent, and each sympathized with the taste of the other. Here Löffler taught them Greek, and young Willdenow, a rising botanist, with perhaps the best herbarium in Europe, instructed them in botany, while Dohn, Engel, Klein and other distinguished professors were engaged by their ever faithful tutor, to give them private lectures in law, politics, philosophy, mathematics, and the physical sciences. Kunth too heard all these lectures, and a little boy of nine, four years younger than Alexander, heard some of them, Leopold von Buch his name.

Thus they passed two years partly at the capitol and partly at Tegel, always under the watchful care of Kunth, and all three under the eye of the mother, with no temporal cares to retard their progress, and no family obscurity to embarrass their position. Two years later Kunth and his charge, late in 1785, took their first leave of the mother for a time, and went to join the University at Frankfort on the Oder, where they resided two years or more, in the family of their Greek professor Löffler, who had removed thither from Berlin. At Easter, 1788, having exhausted the resources of Frankfort and grounded for a higher course, Kunth accompanied the brothers to Göttingen, at that time the most celebrated University of Germany. Here Alexander at the age of eighteen found ample scope for all his aspirations in nature and natural science, and both brothers had more ample opportunities afforded them to follow out the diverse branches of research to which each felt a strong innate tendency.

The University of Göttingen was then at its zenith, with the best selected library of modern books in Europe. Here they met and cemented lasting friendships with those world-renowned Professors, Blumenbach, Eichhorn, Gmelin, Heeren, Heyne and George Forster, all of whom in after life the brothers were never tired of naming with love and gratitude. The wives of Heeren and Foster were the accomplished daughters of Heyne, he of *the* Homer, he of *the* Virgil, who was himself both Professor of languages and librarian of the University. Into this learned family circle the brothers were cordially admitted in the closest terms of intimacy. Alexander soon became the favorite pupil of the great naturalist Blumenbach, and was proud to call himself the scholar of Gmelin.

But it was to George Forster that Alexander never ceased to acknowledge his indebtedness. Forster was then only thirty-

four. He had accompanied Capt. Cooke in his second voyage round the world in 1773–1775, with his father J. Reinhold Forster, and in the following year 1777 gave to the world his marvelous book, *Reise nach der Südsee*, which afterwards made a profound impression on the mind of young Humboldt, and was perhaps the first great incitement in him to the scientific study of nature. How rejoiced then must he have been to find at Göttingen, Forster whose voyages he knew by heart, and whom he delighted long after to call 'my celebrated teacher and friend.' 'If I might,' wrote he in *Cosmos* in the late evening of his life, 'be permitted to instance my own experience, and recal to mind the source from whence sprang my early and fixed desire to visit the land of the tropics, I should name George Forster's *Delineations of the South Sea Islands*,' etc.

The same year, 1788, and while enjoying the society of Forster, there appeared another little book which seems to have still further aroused his love of nature and strengthened his resolution, already formed, for great voyages. This was Bernardin de St. Pierre's master work, *Paul and Virginia*, a copy of which he says in *Cosmos* 'accompanied me to the climes whence it took its origin. For many years it was the constant companion of myself and my valued friend and fellow traveler Bonpland, and often in the calm brilliancy of a southern sky, or when in the rainy season the thunder re-echoed, and the lightning gleamed through the forests that skirt the shores of the Orinoco, we felt ourselves penetrated by the marvelous truth with which tropical nature is described, with all its peculiarity of character, in this little book.'

After a residence of nearly two years at Göttingen, studying languages, botany, zoology, geography, chemistry, mathematics, geology, mineralogy, etc., in the spring of 1790, Humboldt joined his friend Forster at Mayence, whither he had removed, and they two set out on a private scientific Exploring Expedition down the Rhine. At that time the great question that devided geologists, had reference to the Plutonian and Neptunian origin of rocks. The Basalt of that noble river was before him, and accoutred as he was he plunged into the controversy with mind impartial and fresh from the university. The result of his investigations appeared the same year in his first book, at the age of twenty, entitled, *Mineralogische Beobachtungen über einige Basalte am Rhein. Braunschweig*, 1790, 12°. It is a very neat little volume arrranged with taste and judgment, and in a scientific point of view is said to be creditable to a much older head. The book was published anonymously and is now but little known, being very scarce. The copy described in the Catalogue (No. 4563), and now belonging to General Frémont, possesses peculiar interest. It bears the author's autograph sig-

nature, at the end of the dedication, and was presented to Professor Gmelin in 1790, with an affectionate inscription 'by his scholar, A. von Humboldt.' More than sixty years after, on his eighty-fifth birth-day, this precious little volume was re-presented by Theodor Wagener to the great philosopher, who on the 14th of Sept., 1854, inscribed in it a graceful memento of his youth and of his old age.

From the Rhine the travelers passed through Holland and Belgium, and thence to England, where Forster introduced Humboldt to the President of the Royal Society, Sir Joseph Banks, his fellow voyager fifteen years before with Capt. Cooke round the world. He was warmly received by many of the scientific men of London. At the house of Warren Hastings he was shown some pictures by Hodge representing the shores of the Ganges, which made a lasting impression upon his youthful mind, and still further increased his longing for travel. On returning to Germany he published his book, and then the first great question of his life was asked by his mother. What next? There is some pretty strong evidence that she desired him to take to political life and become a diplomatist or statesman. At all events it was determined that he should enter upon some public employment, and the better to qualify him for this, he went to Hamburgh, and in the winter of 1790–1791 attended the celebrated Commercial Academy of Ebeling, studying business, accounts, trade, banking, and commerce, as much as his ardent love of the physical sciences would permit. But even here, of all places in the world, he was unexpectedly encouraged in his ruling passion. Ebeling became his friend and opened to him the treasures of his own private library, at that time one of the largest collections of voyages and travels with maps and geographical works relating to America, then in Europe. Ebeling was about to go to press with the first volume of his great work on America and hence his mind was full of the subject. That rich collection was afterward purchased by Mr. Thorndike of Boston, and presented to the library of Harvard College, of which to this day it forms a prominent feature.

After a struggle of a few months between business and science, Humboldt's inordinate love of the latter finally triumphed, and in 1791 he turned his back on Ebeling his friend, and commerce his foe, and soon after found himself in Werner's house in Freiberg, with that dear boy for his chum whom he had met at lectures eight years before in Berlin, Leopold von Buch, then a youth of seventeen. Werner, at that time the greatest geologist of the age, was Director of the School of Mines at Freiberg. From sight these two young men became friends for life. More than three score years later, wrote Humboldt, 'That I should be destined—I, an old man of eighty-three—to an-

nounce to you, dear Sir Roderick, the saddest news that I could have to convey:' . . . 'Leopold von Buch was taken from us this morning' . . . . 'without him I am desolate.' . . . 'His mind left a track of light wherever it passed.' . . . 'We were together in Italy, in Switzerland, in France,—four months in Saltzburg.' At Freiberg Humboldt devoted himself with humboldtian energy to the study of mining and metallurgy. His mind was ever open and ready for impressions, which it received as surely as wax, and as speedily as photography. No bee could exhaust the wild flowers of the woods quicker than he could extract from his masters all they had to impart. Scarcely a year then sufficed to accomplish his aim at Werner's, whom he left in March, 1792, and returned to his mother at Tegel.

Humboldt had now arrived at the end of his pupilage; and such a pupilage! unparalled in biography. Who before him was ever so favored by fortune, so mentally gifted, so lovingly *led*, and so intellectually prepared, for the brilliant career upon which he was about to enter? Yet he took no royal road to his acquisitions, but that hard paved one open to all, with work, self reliance, energy and love of nature for mile stones. At this early age we find him in scholarship ripe beyond his years, a linguist, an archaeologist, a botanist, a geographer, a geologist, a mineralogist, a metallurgist, a chemist, and an author. His travels for the period had been considerable, and few had so many learned friends. What a contrast to the youthful struggles of the immortal Franklin, and yet these two are perhaps the brightest examples for youth in record. Thus prepared Humboldt was launched into the wide world at the age of twenty-two, burning with an irresistible desire, as he repeatedly tells us in after life, to travel in distant lands unexplored by Europeans.

The next five years, from 1792 to 1797, the young aspirant is tracked with some difficulty through a combination of circumstances well calculated to elevate, strengthen and mature him for the execution of some grand project. Born and educated in central Germany, remote from salt water, with a love of nature ingrain and strengthening with his knowledge, he longed for the sea, as he tells us, and the tropics, and had already resolved, as soon as the opportunity presented itself, to go round the world, and gratify his enthusiasm for the savage beauties of tropical countries guarded by mountains and volcanoes, shaded by primeval forests and watered by vast unexplored rivers; and going or coming, explore that New World where man and his handiworks of ancient and modern civilization had not intervened to dwarf the stupendous display of gigantic nature. All his studies now tended to qualify him for a scientific traveler. As his journey was to be the circle of the globe, so his study

was the circle of the sciences. He worked hard and observed closely; and, what in a young observer of nature is of the highest importance, he reduced to order his observations and wrote them out. During his short year with Werner, the parent of the Neptunian theory, he found time to collect and describe the cryptogamous plants he found growing far down in the mines of Freiberg. He made drawings of them, wrote out their natural history in good Latin, and sent his manuscript to Göttingen to his old friend and teacher, Blumenbach, who soon after returned it, edited with his own notes, and backed with the seal of his approbation. The work,* a handsome quarto volume, saw the light the next year, at Berlin, it being his *second* book, at the age of twenty-three. The second part of it, *Aphorisms on the chemical physiology of vegetables*, he found of great use to him in his observations in America.

This same year he accepted an official position under government in order that he might have influence and opportunities the better to pursue his investigations. He became the Superintendent of Mines in Franconia, and during the short two years he held that position, is said to have remodeled the systems of development and management. He inspected every department himself, near and remote, and became both an executive officer and a business man; while at the same time he was a student, an observer, an explorer and an author. His mental activity was perfectly marvelous, and his scientific and literary labors prodigious. He made experiments and contributed many articles both in his own and in the French language, to the chief scientific journals of Germany and France. Humboldt thus early achieved a reputation throughout Europe as a rising naturalist. In 1795 he resigned this official position, which, although favorable to the cultivation of his favorite pursuits, still did not fill his active mind, now more and more thirsting for explorations in the equatorial regions. This passion led him to devour and analyze the relations of voyages and travels to India, America, Africa and the Islands, but generally to regret the want of variety of knowledge in insulated branches of natural history. The great Expeditions of Fleurieu in 1768–69, of Bougainville in 1766–69, of Cooke 1768–1780 were familiar to him as household words, as were also afterward those of Vancouver, La Pérouse and d'Entrecasteaux; but all these, though they gave ample accounts of the oceans, their islands and their coasts, yet left him unsatisfied as to the vast interiors of countries and continents. They developed

* FLORÆ FRIBERGENSIS Specimen Plantas cryptogamicas præsertim subterraneas exhibens. Accedunt Aphorismi ex Doctrina Physiologiæ chemicæ Plantarum. iv Plates, 4°, Berolini, 1793. The *Aphorisms* the next year, 1794, were translated into German by G. Fischer, with additions by J. Hedwig, and a Preface by G. F. Ludwig, and published at Liepzig in 8vo.

marine geography and nautical astronomy, but left comparatively untouched, physical geography, botany, zoology, the relations of the vegetable world, the migrations of the *social* plants, and the geological structure of mountains and volcanoes. All these he found set forth more to his liking in M. de Saussure's scientific explorations of the Alps and Vesuvius, which interested him profoundly and caused him to study carefully both the results and the use of the instruments by which they were attained.

In this same year, 1795, freed from official care, Humboldt traveled much through Germany and visited Vienna, where he renewed his studies in botany and physical geography, studied and traveled with Freiesleben the celebrated geognost, and with von Haften visited northern Italy, but was for political reasons deterred from going to the volcanic regions of Naples and Sicily. At Vienna he became acquainted with the recent discoveries of Galvani which interested him deeply, and henceforth galvanism became one of his special studies, if indeed a mind of such general grasp can attend on specialties. Many months of most useful preparatory study he passed there examining the exotic plants, and enjoying the friendship, of M. de Jacquier and of M. Vander Schott. Already familiar with the experiments of Franklin and others in electricity, he began there his famous experiments in chemistry, galvanism, electricity and other matters pertaining to organic life, which in importance and originality rivalled the celebrated but subsequent investigations of Bichat.

About this time, at the instigation of Baron von Zach, he found time to acquire a practical knowledge of astronomy, surveying, geometry and mathematics, all so essential to travelers, and became familiar with the use of the various scientific instruments for ascertaining latitudes and longitudes, heights and distances, etc. Next, in the winter of 1796–97, we find him at Jena studying anatomy and physiology under Loder. Here he continued his investigations into animal life in connection with chemistry, galvanism and electricity, and especially experimented on the irritability of the muscular fibre, the vital force in animals and plants, and almost hoped that he had found the clue to the phenomena of life. In close connection with these studies he found it necessary to obtain a practical knowledge of zoology, ichthyology and ornithology. All these investigations and experiments, containing the germs of a new science, which, especially in America, is to-day exerting itself with vigor, were embodied in his *third* book* published in Posen in 1797, in two volumes in octavo.

* Versuche über die gereizten Muskel- und Nervenfasern oder Galvanismus, nebst Vermuthungen über den chemischen Process des Lebens in der Thier- und Pflanzen-

Humboldt now began to think seriously of leaving Europe for a long journey, but regretted to do so without first having seen Vesuvius, Stromboli and Etna, to enable him by comparison to form a 'proper judgment of a great number of geological phenomena; especially of the nature of the rocks of trap formation, it became necessary to have examined strictly the phenomena offered by burning volcanoes.' At Jena he revived his friendship with Goethe and Schiller, the one twenty, the other ten, years his senior, both of whom he terrified by his tremendous energy, and inspired with his own love of nature. 'My natural historic studies have been roused from their winter sleep by his presence' wrote Goethe to Schiller; and wrote Schiller to Goethe 'Although the whole family of Humboldt lie ill of the ague they speak only of great journies.' He therefore determined to return to Italy and with his friend Leopold von Buch set out in November, 1797. They spent some time in Vienna, four months in the several cantons of Salzburg and Styria, pursuing to great advantage their geological investigations; but as they were about to pass the Tyrolian Alps the wars of Italy compelled them to turn back, and, to Humboldt's great regret, to abandon the volcanoes. They then proceeded through France home to Berlin.*

The time had now arrived for immediate preparation for his great voyage. But whither go? He was undecided, the impediments of wars and politics being so great it was impossible to determine. However, having buried his mother, and settled his worldly affairs for a long absence, he set out for Paris in 1798. His fame had already preceded him, and he soon made the acquaintance of many savans, and set about earnestly to collect the instruments and all things necessary for a long scientific exploring expedition. The very list of this apparatus filling pages in his *Personal Narrative* is enough to overpower the mind of an unscientific traveller. There were chronometers, telescopes, (achromatic and simple), lunettes, sextants, reflecting and repeating circles, theodolites, artificial horizons, quadrants, compasses, graphometers, dipping and other needles, magnetometers, pendulums, barometers, thermometers, hygrometers, electrometers, cyanometers, eudiometers, phosphoric eudiometers, boiling water meters, thermometrical leads, areometers, compound microscopes, meters, gauges, chains, tubes, vases, evaporators, Leyden vials, galvanic apparatus, etc., not one of each only, but

welt mit Blumenbachs Anmerkungen 2 vols., viii Plates. 8° Posen, 1797–1799. This work was immediately translated into French with the following title: Expériences sur le Galvanisme et en général sur l'irritation des fibres musculaires et nerveuses: trad. de l'allem. [par Gravel] avec des additions par J. F. N. Jadelot, medicin. 8° Paris, 1799.

* About this time he must have prepared his *fourth* and *fifth* books, which were printed at Braunschweig in octavo in 1799, the one *über die chemische Zerlegung des Luftkreises und über einige andere Gegenstände der Naturlehre*, and the other *über die unterirdischen Gaserten und die Mittel ihren Nachtheil zu vermindern.*

in many instances duplicates and even triplicates. Most of the instruments he had already tested in kind in his various travels and explorations the past two years, and had therefore confidence in his own judgment in selecting them.

The first opportunity that presented itself he accepted, though not much to his taste. Lord Bristol asked him to accompany him to Upper Egypt on an archæological exploring expedition of eight months. He accepted this proposal and for some time directed his studies in conformity with this new project; and though it was abandoned in consequence of the temporary insecurity to travelers, he found that the archæological information then acquired proved in Mexico to be of no inconsiderable service to him. Meanwhile he had made the acquaintance in Paris of two young naturalists, Aimè Bonpland of La Rochelle, and Michaux of Versailles, who had been appointed to the proposed Exploring Expedition to be commanded by Captain Baudin, round the world by Cape Horn, skirting South America from the La Plata to Quito and Panama, and thence across the Pacific to New Holland, Van Diemen's Land, Madagascar (the scenes of his friends Paul and Virginia), and so home by the Cape of Good Hope. Though Humboldt had little confidence in Capt. Baudin, he obtained permission to embark with all his instruments, reserving to himself, however, the liberty to leave the expedition whenever he thought proper. For several months he worked with an eye single to this great enterprise, with his whole heart and soul in it, when, on a sudden, news came that war had broken out in Germany and Italy, and Napolean had determined to postpone the expedition indefinitely. The disappointment was cruel, but the knowledge he had gained was not dissipated. His determination now was to quit Europe at once, by engaging in any enterprise that might tend to console him.

He had made the acquaintance of a Swedish Consul, appointed by his government to carry presents to the Dey of Algiers, passing through Paris, who offered to give to Humboldt, if he would accompany him, the advantage of his long acquaintance in that part of Africa, to facilitate him in visiting the Atlas Mountains of Morocco. No mineralogist had yet examined this lofty chain of mountains which rose to the limit of perpetual snow. He jumped at this proposal, and his friend Bonpland jumped with him. The Swedish frigate was to reach Marseilles towards the end of October, 1798, and therefore all three hastened thither. Two long months they waited there, and no frigate came, but finally news reached them that she had met with accidents and could not be expected at Marseilles till spring. Disappointed again, almost disheartened, but not discouraged, they resolved to spend the winter in Spain, and in the spring if possible embark at Carthagena or Cadiz for the East. Their instruments they took with them, leaving only

the duplicates to Marseilles to follow. Their object was still to work to the East, to India if possible. They crossed Catalonia, Valencia and Old Castile to Madrid, making on their way many astronomical and geographical observations, and ascertained the inclination of the needle and the intensity of the magnetic forces, the results of which were never published.

Immediately on their arrival in Madrid they had reason to rejoice at the wind that wafted them to Spain. Baron de Forell the Saxon minister, himself a mineralogist, at once interested himself in their behalf, thought they might obtain through the enlightened minister Urquijo, permission to visit the interior of Spanish America. The friends hesitated not a moment to adopt this suggestion. In March, 1799, Humboldt was presented at Court, and himself presented to the King a memoir on the motives which led him to undertake a voyage to the new continent and the Phillippine Islands. The result was without delay two passports for himself and Bonpland, one from the Secretary of State, and the other from the Council of the Indies. That he might not be obstructed by narrow-minded viceroys, priests or remote officials of church or state, he had it set down plainly in the bond that he was authorized to make free use of his instruments of all kinds, might make astronomical observations, measure heights and weigh mountains, examine the soil, explore rivers, inspect mines, and in short execute all operations deemed useful for the progress of the sciences, throughout the whole of the Spanish dominions. No passport from the Spanish government, before or probably since, was ever so liberal. Thus after many disappointments, and changes of plans, consequent upon the wars and the generally unsettled state of the political world, Humboldt had great reason to congratulate himself, as indeed the whole world with all the sciences at this day has to congratulate itself, that he at last drifted into Spain, all the tougher, the wiser and the richer for his many disappointments. The travelers proceeded immediately to Coruña, secured a passage in the Sloop Pizarro, a companion of the monthly packet boat, and in June, 1799, embarked their instruments and impedimenta. But the port of Coruña was at that time blockaded by an English squadron. However, under the protection of a friendly storm which obliged the English to stand out to sea, and the cover of a dark night, the Pizarro ran the blockade, and on the sixteenth of July, landed at Cumana in Venezuela, within sight of that beautiful Paria which Columbus discovered in April, 1498, and believed to be Paradise, whence our first parents were expelled. Thus three hundred years after Columbus, an Italian, had sailed from a port in Old Spain to discover a new world, Humboldt, a German, and his friend Bonpland, a Frenchman, availed themselves of the same alien courtesy to go and discover what it contained.

[To be continued.]

ART. I*.—*Livingstone's African Explorations.** *Despatch from Dr. Livingstone to the Earl of Clarendon*, dated 'near Lake Bangweolo, South Central Africa, July 8th, 1868.'

My Lord—When I had the honor of writing to you in February, 1867, I had the impression that I was then on the watershed between the Zambesi and either the Congo or the Nile. More extended observation has since convinced me of the essential correctness of that impression; and from what I have seen, together with what I have learned from intelligent natives, I think that I may safely assert that the chief sources of the Nile, arise between 10° and 12° south latitude, or nearly in the position assigned to them by Ptolemy, whose River Rhaptus is probably the Rovuma. Aware that others have been mistaken, and laying no claim to infallibility, I do not yet speak very positively, particularly of the parts west and northwest of Tanganyika, because these have not yet come under my observation; but if your Lordship will read the following short sketch of my discoveries, you will perceive that the springs of the Nile have hitherto been searched for very much too far to the north. They rise some 400 miles south of the most southerly portion of the Victoria Nyanza, and, indeed, south of all the lakes except Bangweolo.

Leaving the valley of the Loangwa, which enters the Zambesi at Zumbo, we climbed up what seemed to be a great mountain mass, but it turned out to be only the southern edge of an elevated region, which is from 3000 to 6000 feet above the level of the sea. This upland may roughly be said to cover a space south of Lake Tanganyika, of some 350 miles square. It is generally covered with dense or open forest, has an undulating, sometimes hilly, surface; a rich soil; is well watered by numerous rivulets, and, for Africa, is cold. It slopes toward the north and west, but I have found no part of it under 3000 feet of altitude. The country of Usango, situated east of the space indicated, is also an upland, and affords pasturage to the immense herds of cattle of the Basango, a remarkably light colored race, very friendly to strangers. Usango forms the eastern side of a great but still elevated valley. The other or western side is formed by what are called the Kone Mountains, beyond the copper mines of Katanga. Still further west, and beyond the Kone range or plateau, our old acquaintance the Zambesi, under the name of Jambaji, is said to rise. The southern end of the great valley inclosed between Usango and the Kone range is between 11° and 12° south. It was rarely possible there to see a star, but accidentally awaking one morning between 2 and 3 o'clock, I found one which showed latitude 11° 56′ south, and we were then fairly on the upland. Next day we passed two rivulets running north. As we advanced, brooks, evidently perennial, became numerous. Some went eastward to fall into the Loangwa; others went northwest to join the River Chambeze. Misled by a map calling this river in an off-hand manner 'Zambezi, eastern branch,' I took it to be the southern river of that name; but the Chambeze, with all its branches, flows from the eastern side into the center of the great upland valley mentioned, which is probably the valley of the Nile. It is an interesting river, as helping to form three lakes, and changing its name three times in the 500 or 600 miles of its course. It was first crossed by the Portuguese, who always inquired for ivory and slaves, and heard of nothing else. A person who collected all, even the hearsay geography of the Portuguese, knew so little actually of the country that he put a large river here running 3000 feet up-hill, and called it New Zambesi.

I crossed the Chambeze in 10° 34′ south, and several of its confluents south and north, quite as large as the Isis at Oxford, but running faster, and having hippopotami in them. I mention these animals because in navigating the Zambezi I could always steer the steamer boldly to where they lay, sure of

* From the Minutes of the Royal Geographical Soc., for the Meeting of Nov. 8th

finding not less than 8 feet of water. The Chambeze runs into Lake Bangweolo, and on coming out of it assumes the name Luapula. The Luapula flows down north past the town of Cazembe, and 12 miles below it enters Lake Moero. On leaving Moero at its northern end by a rent in the mountains of Rua, it takes the name Lualaba, and passing on N.N.W., forms Ulenge in the country west of Tanganyika. I have seen it only where it leaves Moero, and where it comes out of the crack in the mountains of Rua, but am quite satisfied that even before it receives the river Sofunso from Marungu, and the Soburi from the Baloba country, it is quite sufficient to form Ulenge, whether that is a lake with many islands, as some assert, or a sort of Punjaub —a division into several branches, as is maintained by others. These branches are all gathered up by the Lufira—a large river, which by many confluents drains the western side of the great valley. I have not seen the Lufira, but pointed out west of 11° south, it is there asserted always to require canoes. This is purely native information. Some intelligent men assert that when the Lufira takes up the water of Ulenge, it flows N.N.W., into Lake Chowambe, which I conjecture to be that discovered by Mr. Baker. Others think that it goes into Lake Tanganyika at Uvira, and still passes northward into Chowambe by a river named Loanda. These are the parts regarding which I suspend my judgment. If I am in error there and live through it, I shall correct myself. My opinion at present is if the large amount of water I have seen going north does not flow past Tanganyika on the west, it must have an exit from the Lake, and in all likelihood by the Loanda.

Looking back again to the upland, it is well divided into districts, Lobisa, Lobemba, Ubengu, Itawa, Lopere, Kabuire, Marungu, Lunda or Londa, and Rua; the people are known by the initial 'Ba' instead of the initial 'Lo' or 'U' for country. The Arabs soften 'Ba' into 'Wa, in accordance with their Suaheli dialect; the natives never do. On the northern slope of the upland, and on the 2d of April, 1867, I discovered Lake Liemba; it lies in a hollow, with precipitous sides 2000 feet down; it is extremely beautiful, sides, top, and bottom being covered with trees and other vegetation. Elephants, buffaloes, and antelopes feed on the steep slopes, while hippopotami, crocodiles, and fish swarm in the waters. Guns being unknown, the elephants, unless sometimes deceived into a pitfall, have it all their own way. It is as perfect a natural paradise as Xenophon could have desired. On two rocky islands men till the land, rear goats, and catch fish; the villages ashore are embowered in the palm-oil palms of the West Coast of Africa. Four considerable streams flow into Liemba, and a number of brooks (*Scottice*, "trout *burns*"), from 12 to 15 feet broad, leap down the steep bright red clay schist rocks, and form splendid cascades, that made the dullest of my attendants pause and remark with wonder. I measured one of the streams, the Lofu, 50 miles from its confluence, and found it at a ford 294 feet, say a 100 yards broad, thigh and waist deep and flowing fast over hardened sandstone flag in September—the last rain had fallen on the 12th of May. Elsewhere the Lofu requires canoes. The Louzua drives a large body of smooth water into Liemba, bearing on its surface duckweed and grassy islands; this body of water was 10 fathoms deep. Another of the four streams is said to be larger than the Lofu, but an over-officious headman prevented my seeing more of it and another than their mouths. The lake is not large, from 18 to 20 miles broad, and from 35 to 40 long; it goes off N.N.W. in a river-like prolongation two miles wide, it is said, to Tanganyika: I would have set it down as an arm of that lake, but that its surface is 2800 feet above the level of the sea, while Speke makes that 1844 feet only. I tried to follow the river-like portion, but was prevented by a war which had broken out between the Chief of Itawa and a party of ivory traders from Zanzibar. I then set off to go 150 miles south, then west, till past the disturbed district, and explore the west of Tanganyika; but on going 80 miles I found the Arab party, showed them a letter from the Sultan of Zanzibar, which I owe to the kind offices of his Excellency Sir Bartle Frere, Governor of Bombay

and was at once supplied with provisions, cloth, and beads; they showed the greatest kindness and anxiety for my safety and success. The heads of the party readily perceived that a continuance of hostilities meant shutting up the ivory market, but the peace-making was a tedious process, requiring 3½ months. Iwas glad to see the mode of ivory and slave-trading of these men, it formed such a perfect contrast to that of the ruffians from Kilwa, and to the ways of the atrocious Portuguese from Tette, who were connived at in their murders by the Governor D'Almeida.

After peace was made I visited Msama, the chief of Itawa; and, having left the Arabs, went on to Lake Moero, which I reached on the 8th September, 1867. In the northern part Moero is from 20 to 33 miles broad. Further south it is at least 60 miles wide, and it is 50 miles long. Ranges of tree-covered mountains flank it on both sides, but at the broad part the western mountains dwindled out of sight. Passing up the eastern side of Moero we came to Cazembe, whose predecessors have been three times visited by Portuguese. His town stands on the northeast bank of the lakelet Mofwe; this is from two to three miles broad and nearly four long. It has several low, reedy islets, and yields plenty of fish—a species of perch. It is not connected with either the Luapula or Moero. I was forty days at Cazembe's, and might then have gone on to Bangweolo, which is larger than either of the other lakes; but the rains had set in, and this lake was reported to be very unhealthy. Not having a grain of any kind of medicine, and, as fever, without treatment, produced very disagreeable symptoms, I thought that it would be unwise to venture where swelled thyroid gland, known among us as Derbyshire-neck, and elephantiasis (scroti) prevail. I then went north for Ujiji, where I have goods, and, I hope, letters; for I have heard nothing from the world for more than two years: but when I got within 13 days of Tanganyika, I was brought to a stand-still by the superabundance of water in the country in front. A native party came through, and described the country as inundated so as often to be thigh and waist-deep, with dry sleeping places difficult to find. This flood lasts till May or June. At last I became so tired of inactivity that I doubled back on my course to Cazembe.

To give an idea of the inundation which, in a small way, enacts the part of the Nile lower down, I had to cross two rivulets which flow into the north end of Lake Moero; one was 30, the other 40 yards broad, crossed by bridges; one had a quarter, the other half a mile of flood on each side. Moreover, one, the Luao, had covered a plain abreast of Moero, so that the water on a great part reached from the knees to the upper part of the chest. The plain was of black mud, with grass higher than our heads. We had to follow the path which, in places, the feet of passengers had worn into deep ruts. Into these we every now and then plunged and fell, over the ancles in soft mud, while hundreds of bubbles rushed up, and, bursting, emitted a frightful odor. We had four hours of this wading and plunging—the last mile was the worst; and right glad we were to get out of it to the sandy beach of Moero and bathe in the clear tepid waters. In going up the bank of the lake we first of all forded four torrents, thigh deep; then a river 80 yards wide, with 300 yards of flood on its west bank, so deep we had to keep to the canoes till within 50 yards of the higher ground; then four brooks, from 5 to 15 yards broad. One of them, the Chungu, possesses a somewhat melancoholy interest, as that on which poor Dr. Lacerda died. He was the only Portuguese visitor who had any scientific education, and his latitude of Cazembe's town on the Chungu being 50 miles wrong, probably reveals that his mind was clouded with fever when he last observed, and any one who knows what that implies will look on his error with compassion. The Chungu went high on the chest, and one had to walk on tiptoe to avoid swimming. As I crossed all these brooks at both high and low water, I observed the difference to be from 15 to 18 inches, and from all the perennial streams the flood is a clear water. * * * * * * * * * *

ART. II.—*On the relation between the Intensity of Light produced from the combustion of Illuminating Gas and the volume of Gas consumed;* by B. SILLIMAN.*

(Read at the Salem meeting of the Am. Assoc. for Adv. of Science, Aug. 1869.)

IN photometric observations made to determine the illuminating power or intensity of street gas, it is the practice of observers to compute their observations upon the assumed standard of five cubic feet of gas, consumed for one hour, and in the constantly occurring case, of a variation from this standard, whether in the volume of the gas consumed or in the weight of spermaceti burned, the observed data are computed by the "rule of three," up or down, to the stated terms. The standard spermaceti candle is assumed to consume 120 grains of sperm in one hour, a rate which is rarely found exactly in actual experience.

For example, a given gas, too rich to burn in a standard argand burner at the rate of five cubic feet per hour without smoking, is consumed at the rate of $3\frac{1}{2}$ cubic feet to the hour, with an observed effect of 20 candles power. This result, previously corrected by the same rule for the sperm consumed, is then brought to the standard of five cubic feet by the ratio $3{\cdot}5 : 20 = 5 : 28{\cdot}57$

The candle power of the gas is therefore stated as 28·57 candles, and this result has been universally accepted as a true expression of the intensity of the gas in question, or the relative value of the two consumptions.

In common with other observers, I have long suspected that this mode of computation was seriously in error, as an expression of the true intensity of illuminating flames, and that there were other conditions besides the volume of gas or weight of sperm consumed which must influence, and greatly modify the results. As most of these conditions are considered somewhat at length in a paper on "*Flame Temperatures*," prepared chiefly from researches conducted by Prof. Wurtz and myself, and presented at the Salem Meeting of the Association, they need not be discussed in this connection.

The results of many trials, made with the purpose of determining the value of these photometric ratios, indicate clearly that the true ratio of increase in intensity in illuminating flames is, within certain limits, expressed by the following theorem, viz :—

* The main points of this paper were made the subject of a verbal communication to the Conn. Academy of Arts and Sciences at their session, June 17th, 1869.

The intensity of gas flames, i. e., *illuminating power, increases* (within the ordinary limits of consumption) *as the square of the volume of the gas consumed.*

As the first experimental demonstration of this theorem was made by Mr. William Farmer, the photometric observer at the Manhattan Gas Co's. works in New York, I propose to speak of it as "Farmer's theorem." I am also indebted to Mr. Farmer and to Mr. Sabbaton, the well known and courteous Engineer of the Manhattan Gas Light Company, for the free use of their experimental data and the permission to employ them in illustration of Farmer's theorem.

The fundamental importance of this new mode of computation will at once appear, if, assuming it for the sake of illustration to be true, we apply it to the case already given above, which then becomes—

$$3{\cdot}5^2 : 20 = 5^2 : 40,$$

showing an increase of forty per cent over the old rule of correction. Let us see how far this theorem is sustained by the test of experiment.

Experiment 1*st.*—Two similar gas flames, one at each end of the photometer bar, were made to give exactly the same intensity of illumination. This was accomplished of course by placing the Bunsen disc midway between the two burners, and regulating the combustion until the disc was perfectly neutral; the consumption being noted equal by two wet gas meters under the same pressure. The screen was then moved upon the bar to a point just four times as far from one flame as it was from the other, i. e., the bar being 100 inches, the screen stood at 80, i. e., as 1 : 4. The light from the distant burner was then increased until the disc again showed an equality of illumination. On reading the rate of the gas consumed by the two burners respectively, one gave 3·66 cubic feet and the other 7·32 cubic feet, or exactly double, or in other words, the lights were as the squares of the volumes of gas consumed, thus: $3{\cdot}66^2 : 7{\cdot}32^2 = 1 : 4$.

By the old rule the intensity would have been estimated directly as the volume of the gas consumed, thus:

$$3{\cdot}66 : 7{\cdot}32 = 1 : 2.$$

Experiment 2*d.*—The following results were obtained with the use of a standard argand burner. The readings of the index meter, the gas consumed in cubic feet, and the ratio of the lights produced, are given in three columns, viz:—

Index,	·0550	=	3·30 feet.	=	1	light.
"	·0725	=	4·35 "	=	2·1	"
"	·0856	=	5·13 "	=	3·2	"
"	·0926	=	5·55 "	=	4·	"

In this series the lights increase in considerably higher ratio than is required by Farmer's theorem, which demands 6·60 cubic feet, corresponding to a four-fold consumption, while the actual consumption was 1·05 cubic feet less than the quantity required by the theorem.

Experiment 3*d*.—The following series was obtained by another argand burner.

Index,	·062	=	3·72	cub. feet.	=	1 light.
"	·0814	=	4·88	"	=	2 "
"	·1000	=	6·00	"	=	3 "
"	.1203	=	7·219	"	=	4 "

In this series the ratio is more nearly in accordance with the demands of the theorem, the intensity being still a little in excess of the squares of consumption ($3·72 \times 2 = 7·44$ in place of 7·219).

The gas employed in these comparisons had a candle power of about 14 candles.

Experiment 4th.—Results obtained by a comparison of fish-tail burners, ratio as 4 and 9 feet respectively.

A.	Index,	·0750	=	4 500	cub. feet.	= 1 light.
B.	"	·1586	=	9·519	"	= 4 "

In this comparison the ratio falls but little short of the demands of the theorem.

Experiment 5th.—Comparison of fish-tail burners.

A.	Index,	·086	=	5·16	feet.	= 1·85
B.	"	·1677	=	10·06	"	= 4·

In this trial the departure from the requirements of the theorem is considerably greater than in any of the preceding experiments. But it appears that from some cause the ratio of the squares does not hold with gas of the power used in these trials (14 candles), where the consumption rises above 9 or falls much below 3 cubic feet. This is undoubtedly connected with the well recognized fact, that there is for each gas a kind of burner and a volume of gas better calculated than any other to develop its maximum intensity.

Experiment 6th.—This series was designed by Mr. Farmer to test by a direct comparison the value of the new as contrasted with the old method of correction. Both trials were made upon the same gas, the second observation following immediately after the first and with the same candle, and therefore should give about the same candle power.

1*st Trial*.—Consumption	of sperm,	32·7 grains.
	" of gas,	5·004 cubic feet.
Mean candle power (15 observations),		13·93 candles.

2d Trial.—Consumption of sperm, 32·2 grains.
" " gas, 4·58 cubic feet.
Mean candle power of 15 observations, 11·8 candles.

The above data calculated by Farmer's Theorem.

5·004 cubic feet, and 32·7 grains give 15·15 candles.
4·58 " " 32·2 " " 15·09 "

Difference, ·06 "

Calculated by the old rule.

5·004 cubic feet and 32·7 grains give 15·16 candles
4·58 " " 32·2 " " 13·82

Difference 1·34 "

It is obvious from the study of these results, that within the limits named the increase of intensity in gas flames, whether naked or argand, is at a ratio certainly as great as the squares of the volumes of gas consumed; and hence it follows that all the photometric determinations, which have been obtained by computation from volumes greater or less than the assumed standard of five cubic feet per hour, in the simple ratio of the volumes consumed must be considered as *absolutely worthless*, provided the theorem of Farmer here announced is confirmed.

It is evident also that this theorem applies with equal force to the weight of sperm consumed by the standard candle as to the volumes of the gas burned in equal times.

With a view to test the theorem of Farmer, I at once sought to apply it to the case of certain observations I had made upon very rich gas obtained from cannel and other rich coals. The photometric power of these gases had been measured in the usual way heretofore practiced by gas engineers, by burning a less quantity than five cubic feet in the standard argand and then computing up to a standard of five cubic feet by direct ratio. The results of this comparison appear to go far to confirm Farmer's theorem.

Peytona Gas.—This gas was made from a coal of West Virginia, known as Peytona Cannel Coal. It was much too rich to permit the flow of five cubic feet from the 15 hole argand burner, with a perfect combustion. The gas was therefore reduced by mixture with a measured volume of street gas of known value, and the illuminating power of the mixture having been carefully determined, the value of the Peytona gas alone was readily calculated and fixed at 42·79 candles. The following trials exhibit the result obtained by burning smaller volumes of Peytona gas, and the values obtained by the two methods of calculation.

No. 1 Argand	burner	consuming	5	cubic feet	per hour,	mixed gas=	42·79	candles
2	"	"	3.24	"	"	"	18·95	"
3	"	"	3·48	"	"	"	20·94	"

Here No. 1 represents very nearly the true illuminating power of the gas, and may be assumed as a fair criterion of the law under consideration.

By Farmer's Theorem.

No. 2 becomes $3.24^2 : 18·95 = 5^2 : 45·12$ candles.
3 " $3·48^2 : 20·94 = 5^2 : 43·22$ "

By direct ratio (old rule).

No. 2 becomes 3·24 : 18·95 = 5 : 29·24 candles.
3 " 3·48 : 20·94 = 5 : 30·09 "

By this it appears that by the old rule, assuming the true candle power of the gas to be 42·79 candles, the two observations Nos. 2 and 3 are in error by about 30 per cent, while by Farmer's theorem the error is reduced to 3 per cent, the former being too small and the latter too large.

Albert Gas.—The well known *Albertite* of New Brunswick furnishes a gas of remarkable richness. Its true candle power can be measured only by diluting largely with street gas of known value, and calculating it from the determined intensity of the mixture. In this way the gas from Albertite is shown to have an intensity equal to 70·38 candles. The following results were obtained by consuming different volumes in the burners named.

No. 1	argand	burner	consuming	5	cubic feet	= 70·38	candles.
2	"	"	"	2·25	"	= 16·39	"
3 'Scotch tip'	"	"	3·	"	= 25·25	"	

By Farmer's Theorem.

No. 2 becomes $2·5^2 : 16·39 = 5^2 : 65·56$ candles.
3 " $3^2 : 25·25 = 5^2 : 70·14$ "

By simple ratio.

No. 2 becomes 2·5 : 16·39 = 5 : 32·78 candles.
" 3 " 3 : 25·25 = 5 : 42·08 "

The differences from the assumed standard of 70·38 candles are as follows :

By the old rule,	No. 2	falls short	37·6	candles	or 115	pr. ct.
" Farmer's theorem,	"	"	4·72	"	7·1	"
" the old rule,	No. 3	"	28·30	"	67·25	"
" Farmer's theorem,	"	"	0·24	"	0·34	"

It will be observed that No. 2 in this series represents a consumption considerably below the minimum which in most cases experiment has shown to be the limit of the proposed

theorem, namely, 3 cubic feet, while No. 3, which represents exactly this limit, brings the result within the range of experimental error—it being impossible to make two series of 15 photometric observations which will accord more closely than these.

Wollongong Gas.—This gas was obtained from *wollongonite*, a new carbohydrogen described by me in a late number of this Journal (July, 1869), as coming from Australia. Its illuminating power was determined by mixing 10 per cent of the gas with 90 per cent of street gas. But this mixture was still too rich to burn 5 cubic feet in the Argand standard without smoking, and even when burned at this rate in a fish-tail burner the flame was somewhat smoky and inclined to "tail off." I have therefore little doubt that its true candle power is more nearly 142 candles than to 132 as stated in the article referred to. We quote, however, the observations made as follows:

1 fish-tail burner consuming 5 cubic feet gave 132·94 candle power.
2 " " 1·5 " " 12·89 "

Computing the second observation we have:

By Farmer's theorem for No. 2 143·22 candle power
" direct ratio " " 42·96 " "

This is an extreme case in which the volume of gas consumed in the second observation is far too low, but it is clear that by the old rule the result coming from the consumption of so small a volume of gas is perfectly worthless, while by Farmer's theorem the difference of 10·28 candles is within 7·7 per cent, while if the true intensity of this remarkable gas is placed, as there is good reason to believe it should be, at 142 candles, the agreement in the two observations is absolute.

Every photometric observer can confirm the results here given by reference to his own records of former observations, or by direct experiment designed to test the accuracy of the theorem here announced.

In Sugg's "Gas Manipulation" (London, 1867), page 64, is a tabular statement of the results of an experiment designed to illustrate the unfitness of the "Birmingham" burner, (a special form of argand) to develop the highest intensity of which a gas of 14 candle power is capable when it is used upon any other standard than that of a consumption of five cubic feet per hour. By this statement the burner in question produced from five cubic feet of gas exactly 15 candle power. But when reduced to 4·5 cubic feet consumption the candle power when "corrected to the standard quality of gas by proportion," was only 11·93 candles. The values of the 'correction' referred to

can only be conjectured, but assuming that the observation made the uncorrected rendering 11·32 candles (a very probable quantity) we find that the law of the squares of consumption then makes the ratio as follows :—

$$4{\cdot}5^2 : 11{\cdot}32 = 5^2 : 14,$$

a result which in view of the facts before given cannot be regarded as accidental. The theorem applied to this case as it stands reported (including the correction) gives for the value of the fourth term of the ratio 14·7 candles.

I have endeavored to apply this theorem to some of the results recorded in the well known researches of Messrs. Audouin and Bérard, but I find these results stated in a manner which renders it difficult to fix clearly the terms of comparison. I venture, however, to append a few comparisons drawn from two of the tabular records of experiments with butterfly or bat's wing burners of the "fifth series" which so far as they go lend confirmation to the views here presented.

Burner of the fifth series—slit $\frac{1}{50}$ inch wide.

Consumption of the Burners under trial.	Consumption of the Bengel Argand standard burner without cone; 8 in. chimney.	Comparative intensities. The Bengle burner=100.	Intensities by law of the squares of consumption.	Pressures.
Cubic feet.	Cubic feet.			
3·1079	3·6024	50	103	·23622
2·4015	3·5318	40	90·9	·19685
2·0131	3·6024	30	96·	·11811
Burners of same series—slit $\frac{1}{50}$ inch wide.				
3·9555	3·6730	80	92·6	·078474
3·1786	3·6730	60	80·7	·07480
2·6487	3·6730	50	96·7	·07480
2·3309	3·6730	40	97·5	·03937
1·5186	3·6730	20	115·6	·01968

The comparison of their results by this theorem, which gives reasonably exact results for consumptions which are not greater than that of the standard Bengle burner employed by them, fails when the consumptions become greater than that of the standard.

A comparison of the foregoing results will show that the coincidences with the requirements of the theorem of Farmer are, within the limits assigned, too numerous, and too closely accordant, to be considered as otherwise than pointing clearly to its general truth. A rigorous demonstration cannot be expected, as there are too many variable functions of unknown value involved in the best methods at present known for photometric

measurements to permit more than an approximate proof of its general accuracy. Every photometric observer must recognize its importance and the necessity in his observations of bringing the consumptions of gas and sperm to the agreed standard.

To the consumer of gas the evident inference from the data here presented is that, where it is important to obtain a maximum of economical effect from the consumption of a given volume of illuminating gas, this result is best obtained by the use of burners of ample flow.

Where a moderate light of equal diffusion is required over a large space, as in public rooms, it may be expedient to use numerous small jets; but when the maximum intensity obtainable from a given volume of illuminating gas is desired, intensity burners of large consumption are plainly indicated.

In the discussion following this paper, Mr. F. Stimpson, State Inspector of gas for Massachusetts, brought forward some results of observations he had made upon Farmer's theorem (having been in correspondence with Prof. Silliman on the subject), and considered them in comparison with those herewith given. His conclusion was that while in many cases the theorem was closely applicable, in others it was not so. Mr. Stimpson's discussion of the matter will very likely appear in an early number of this Journal.

ART. III.—*Principles of Molecular and Cosmical Physics*; by Prof. W. A. NORTON.

IN a memoir on Molecular Physics, published in 1864–66, in this Journal, and republished in the London Philosophical Magazine, I showed that, by the introduction of a new hypothesis not in itself improbable, the principle of gravitation might be explained. I now find that no additional hypothesis is necessary but that this principle is essentially involved in one of the fundamental principles of the general theory; viz., that of the *interception of force by matter*. This fundamental principle was brought out in treating of the subject of the electric condition of molecules, and formed an essential element in the special theory of electricity developed in the memoir. This interception was regarded as no arbitrary assumption, but as a necessary consequence of the fact that a certain portion of the propagated force is instantaneously expended in imparting motion to the molecule, or atom, which it encounters, and is therefore abstracted from this force. In every instance of a propagated force this conclusion is inevitable. Now I find that if we admit that the primary forces of

nature are propagated forces, that is, do not act instantaneously at all distances, the principle of universal gravitation, as well as the doctrine of the molecular forces and agencies set forth in my paper, may be directly deduced from *a single force of repulsion exerted by every primary atom upon every other atom.*

Admitted Principles.—It is now universally conceded: (1st,) that matter exists in at least two different fundamental forms, or conditions, viz: that of universal or luminiferous ether which pervades all space, and that of ordinary matter directly recognizable by our senses.

(2d.) That all masses of matter of sensible extent are made up of distinct atoms.

(3d.) That every atom is essentially inert or incapable of itself of altering its own state, whether of rest or motion; and that in every act of motion, or change of motion of an atom, an amount of force is expended proportionate to the mass of the atom and the velocity, or change of velocity, produced in the direction in which the force acts.

It is also the general conception with physicists, that every atom has a definite form, and a definite size dependent upon the quantity of matter which it contains, and it will serve to fix our ideas to adopt this conception; at the same time it should be understood that in order to arrive at our conclusions the only essential supposition to be made, with regard to the state of an atom, is that it occupies a certain space, proportionate to its quantity of matter, in such a manner as to receive and intercept a certain portion of the force propagated along every line, or a number of lines proportional to its mass, traversing this space.

Cosmical Force of Repulsion.—The fundamental notion of the propagation of force involves with it the conception that the force acts, or is transmitted in a series of recurring impulses. We may also assume that, like all known propagated actions, the force varies according to the law of inverse squares. In fact, if the impulses are transmitted along definite lines, and the atom occupies, as a cause of interception, a definite space, it is obvious that this law must of necessity hold good; or, if they are propagated by the intervention of wave pulses in a more subtile ether, whether the atom be regarded as a mere point or of definite size, so be that it has a definite degree of inertia, the same law should obtain. Now, let us leave out of view, for the present, all the bodies of ordinary matter in existence, and confine our attention to the luminiferous ether uniformly disseminated through space. Every atom of this ether exercises a repulsive action upon every other atom of the same at all distances; and this action consists in a series of impulses perpetually renewed, at an immensely more rapid rate, we must suppose, than those of light or radiant heat. Each effec-

tive impulse is but an excessively minute fraction of each individual impulse propagated in the force; and this is expended in giving motion, or virtual motion to the atom. That is, as in all known instances of propagated action, each impulse produces its effect, in the shape of velocity instantaneously imparted, and is expended in so doing, just as if no other impulse or series of impulses were in operation. Since the ether is uniformly disseminated, each atom must be subjected, in an appreciable interval of time, to the same amount of force transmitted from every direction, and therefore cannot experience any sensible displacement. But, although no sensible progressive motion of the ethereal atoms can result, under the circumstances supposed, still it is to be observed that the united effective impulses that come into operation upon the atom in any minute interval of time, *from any one direction*, must be enormously great in comparison with the force of repulsion subsisting between two contiguous atoms of the ether: and this latter force must be vastly greater than the elastic force of the ether called into play in the propagation of a wave of light or heat; since this elastic force results from a slight inequality in the repulsive actions of the contiguous atoms on different sides, attendant upon a slight relative displacement of the atoms. It is, as I conceive, by the coming into operation under certain circumstances, of a portion of this vast cosmical, ethereal force, received from definite directions, that the known effective forces of nature are brought into play.

Immediate consequences of the Interception of the Cosmical Force.—Universal Gravitation.—Let us next conceive that a single atom of ordinary matter is posited at a certain point in the universal sea of ether, and consider what forces would be brought into operation by its interceptive action. In the first place it is obvious that the entire force transmitted from all the ethereal atoms lying on any line intersecting the atom supposed, will be partially intercepted in passing the atom, and that the residual force propagated on will be of less intensity than the force coming toward it in a directly opposite direction. If then any point in space be taken, at any distance from this atom, and right lines be conceived passing through this point and the various points of the atom, and extending indefinitely in both directions, they will form a cone circumscribing the atom and having the point supposed for a vertex; and the force transmitted to the point along every line within the surface of this cone that encounters the supposed atom of ordinary matter, will be less than that received in the directly opposite direction. Accordingly there will be a greater amount of repulsive force transmitted through the vertex of the cone, from the ethereal atoms lying within

the outer nappe or indefinite portion of the cone, than from those lying within the other nappe which circumscribes the atom of ordinary matter. The excess will operate as an effective force to urge any atom that may be posited at the vertex toward the ordinary atom. It is obvious that except at minute distances, this effective force would vary inversely as the square of the distance.

All this being understood, let us consider what will be the result with regard to the ether in the immediate vicinity of the supposed atom of ordinary matter. It is plain that each ethereal atom so situated will be urged toward the atom considered, until the effective force directed inward is neutralized by the outward repulsion of the ordinary atom and the increased repulsion of the ethereal atoms lying nearer to this. The ultimate result then would be the condensation of an ethereal atmosphere around the atom of ordinary matter, just as if this exerted a direct attractive action upon the adjacent ether. Now what will be the effect of the supposed atom (*a*), and its condensed atmosphere of ether, upon any other atom (*b*)? In the first place the entire interception of the cosmical repulsion produced by *a* and its atmosphere would give rise to a gravitating tendency of *b* toward *a*. But *a* and its atmosphere exert a direct repulsive action upon *b*. This direct force of repulsion is the sum of all the repulsive impulses exerted by *a* and the atoms of its atmosphere. But for the interception of individual pulses, produced by *a* and the atmospheric atoms encountered on lines of propagation, it would be the same as if all the repellant matter considered were concentrated at the center of *a*. As a matter of fact, in consequence of this interception, the center of repulsion will be displaced toward the exterior atom *b* acted on. Beyond a certain minute distance the direct repulsion thus exerted upon *b*, will vary inversely as the square of the distance from *a*; and it will at the same time be less, at all distances, than the gravitating tendency originating in the manner above explained.

The excess of this tendency toward the atom (*a*) *of ordinary matter above the repulsion exerted by the atom and its atmosphere, constitutes the effective force of gravitation due to the atom.* It will vary, at all measurable distances, according to the law of inverse squares.

Before inquiring into the dependence of this force upon the quantities of matter in the active atom and in that acted upon, we will remark that nothing precludes us from supposing that *the atom of ordinary matter, so called, is actually a mass of condensed ether.* It will readily be seen that if a certain portion of the equally diffused ether lying around any point were to be condensed upon this point, the increased interception of the

cosmical force within the space that it would occupy, would develop, as above explained, an inward acting force that might be in equilibrio with the augmented mutual repulsion of the atoms of the condensed ether. If this conception be adopted, the interception of the cosmical force, effected by an atom of ordinary matter, will take place at each of the ethereal atoms of which it is composed, and the entire effect will be proportional to the number of such atoms, or the entire mass of the compound atom. If then a second such atom of ordinary matter (B) be considered, at a distance from the first (A), the effective gravitating tendency of each of its constituent ethereal atoms toward A will be proportional to the mass of A. The entire gravitating tendency of B toward A, will then be proportional to the mass of A multiplied into the mass of B. Also, since the tendency of each ethereal atom of B toward any ethereal atom of A is inversely proportional to the square of the distance, the same law will hold for the entire gravitating tendency of B toward A. It is to be borne in mind that the effective force of gravitation here considered, is the excess of the gravitating tendency due to the partial interception of the general cosmical force by the atom A and its ethereo-electric atmosphere, over the repulsion directly exerted by the same.

The Newtonian principle of gravitation being thus made out for individual atoms of ordinary matter, it is also made out for cosmical masses. The Newtonian laws of the mutual gravitation of masses, in all their precision, are inevitable consequences of the principle of gravitation as now deduced from the one cosmical force of repulsion; if we admit that the portion of every impulse of this force that is intercepted by each atom is an excessively minute fraction of the whole intensity of the propagated impulse.

Molecular Forces.—The Molecular Forces, so called, consist of a force called the attraction of cohesion, and one or more forces of mutual repulsion. The force of heat is recognized as one force of repulsion, and has generally been regarded as the only repulsion in operation. But this notion must certainly be discarded by all physicists who maintain that the heat-repulsion is due to vibrations of the atoms of bodies; since such vibrations can only be maintained by the operation of existing antagonistic forces of attraction (or virtual attraction) and repulsion. And even if we admit, as I cannot do, the mechanical possibility of molecular motions of revolution, or rotation, so constituted as to originate in some way a force of heat-repulsion exerted in all directions outward from each atom, and to be always augmented in rapidity by waves of radiant heat from whatever direction received, it is wholly inconceivable that in the collision of bodies such motions of revolution or rotation of

the impinging atoms, or molecules, should give rise to a mutual repulsion, whatever might be the relative direction of the motions. In fact, in whatever mode of molecular motion heat may be supposed to consist, if heat be the only force of repulsion, a certain amount of the living force of heat belonging to each of two impinging bodies would be expended in diminishing the velocity of the one body and augmenting that of the other, and it would be impossible that they should become heated by the impact. It is here assumed that the impact of two bodies must develop a force of mutual repulsion between the impinging molecules, which determines the equality of the action and reaction. This obvious fact seems now-a-days to be in a good degree ignored, and the exchange of momenta to be supposed to be brought about by some unimaginable process, in which the idea of force is wholly lost sight of.

We must then conclude that there is a primary force of molecular repulsion, in addition to that of heat. We might perhaps ascribe this force to the repulsion of the ethereal molecular atmospheres when brought into contact; but to what can we ascribe the heat-repulsion? It has come to be generally believed that it must consist in some mode of motion of the atoms, or molecules of bodies, as a whole; either of vibration, revolution, or rotation. But it might be almost demonstrated, did space permit, that this cannot be the true nature or origin of heat. I will only allude here to one or two arguments in support of this statement, which may be briefly given. It is well known that whenever any body is by collision with another body, or in any other way, permanently compressed, heat is given out. Now the fact that a force of pressure, or percussion, produces a permanent compression, increasing with its intensity, leads to the almost inevitable inference that in the act of compression the atoms, or molecules, experience some change of physical condition, by reason of which the molecular attraction is augmented, and the condensation maintained, and that this change must be proportionate to the degree of condensation, and so to the amount of heat evolved. Can such a change proportionate to the heat evolved, be connected by any admissible supposition with the augmented velocities of vibration, revolution, or rotation of ordinary atoms, in which the increase of heat is conceived to consist? In fact the entire phenomena of inelastic, as well of elastic reaction, point to the conclusion that they are attended with changes in the physical condition of the atoms, ordinarily so called, and that the evolution or absorption of heat that takes place is the direct result of these changes.

Again it is not conceivable that the phenomena of electricity can be ascribed to any imagined motion of ordinary atoms, or molecules, whether these are surrounded with ethereal atmos-

pheres or not. We are accordingly constrained to look in some other direction for the origin of the molecular forces and agencies; and it appears to me that it is to be found in the general conception of an electric ether less subtile than the luminiferous, and gravitating like this toward the atoms of ordinary matter, so as to form envelopes to these atoms, permeated and underlaid by the condensed luminiferous ether. Each atom would thus be surrounded by an ethereo-electric atmosphere. The atoms of electric ether may be conceived to be masses of condensed luminiferous ether, but far more minute than those of ordinary matter. Adopting this conception of the existence of an electric ether, we are led, by legitimate inferences from the fundamental principles I have laid down in this paper, not only to the physical constitution of a primitive molecule, but also to the theory of molecular forces set forth in my memoir on Molecular Physics. The force of molecular attraction was conceived to consist in a contractile action exerted by the central atom upon its electric envelope, and originating waves propagated outward through the electric ether to contiguous atoms. This contractile action is now seen to consist in the gravitating tendency of the electric envelope toward the central atom; resulting from the partial interception of the cosmical force by this atom. This action also originates waves in the luminifereous ether posited between the central atom and its electric envelope, that are propagated outward by this ether, and constitute the primary force of heat-repulsion. All the diverse methods of developing heat are but different methods of compressing or forcing inward the atomic or molecular envelopes, and so originating ethereal heat-waves. These waves at the outset pulsate in the line of propagation; but, by passing around the ethereal atmospheres of molecules, become converted into waves attended with transverse vibrations. The other force of molecular repulsion originates in the direct repulsion subsisting between the diverse atoms of the electric envelope. This force originates waves that proceed outward through the electric ether from different depths in the envelope. These waves must increase in their intensity at the outset from the lowest depth upward, to a certain height in the envelope, by reason of the increase in the quantity of ether that is effectively repellant. On the other hand the attractive waves, that issue from various points of the envelope, must decrease outward in their intensity at their origin. It thus happens that the resultant waves, which may be taken to represent the entire actions of these two systems of waves, must be conceived to proceed from different depths; which depths may therefore be taken as the upper and lower limits of the effective envelope. This is equivalent to one of the principles adopted in the mathematical theory of the

molecular forces developed in my former memoir; viz. that the attractive waves proceed from the lower limit of the electric atmosphere, or envelope, and the waves of electric repulsion from the upper limit. In the memoir referred to it was shown that the entire action of one atom, or rather primitive molecule, on another, amounts to a repulsion at the more minute distances, and an attraction at greater distances up to a certain limit, beyond which it becomes a repulsion to an indefinitely great distance. The first mentioned, or inner repulsion, and the attraction, are the forces which determine and maintain the solid and liquid condition of matter, and the outer repulsion constitutes the expansive force of vapors and gases. The extraneous heat that a body may receive is a distinct force of repulsion, modifying the natural curve of molecular action.

Origin of Chemical Attraction, and of Electric and Magnetic Forces.—The electric envelopes of atoms, besides being the source of the molecular forces, including the primary heat-repulsion, invests the molecules of each substance with the property of chemical attraction for the molecules of other substances to which they bear a certain physical relation. This consists in the disturbance of the equal distribution of the ether in the envelopes of two contiguous heterogeneous molecules, under their natural molecular action, by reason of which the molecules become oppositely polarized on their adjacent surfaces—the one having an excess of the electric fluid and the other a deficiency. The attraction thus originating may prevail over the natural molecular repulsion that comes into play when the two liquids come into contact, and bring the molecules within the range of their natural attraction.

The state of molecular electric polarization, superinduced under certain varied circumstances, originates the galvanic current, and determines the phenomena of electro-static, electro-dynamic, and magneto-electric induction. The attraction and repulsion of electric, and of magnetic currents, may be ascribed to the origination and maintenance of currents in the adjacent body of luminiferous ether, running in the same direction as the electric or magnetic currents. These currents determine inequalities of elastic force in this moving mass of ether, from which result *transverse currents*. These transverse currents take effect upon the line of the electric or magnetic current because they are partially intercepted by it. The induction of one electric or magnetic current by another, is effected by the intervention of currents directly induced in the adjacent body of luminiferous ether.

The key to the explanation of the excitation of electricity by friction lies in the fact made out in my former paper, that

the electric ether is condensed between the component molecules of each compound molecule of a substance; and that every expansion of the compound molecule sets free a certain portion of this condensed ether, and every condensation withdraws a certain portion from adjacent molecules that are undisturbed. If two dissimilar surfaces are rubbed, the one over the other, the consequent disturbance of the compound molecules of the two surfaces, will be either unlike or unequal in amount; and a certain quantity of electricity will in consequence pass from the one surface to the other. As soon as the friction ceases the disturbed molecules will recover their original form and size, and the positive state of the one surface and the negative state of the other will manifest themselves. For example, if the molecules of the one surface are compressed by the rubbing, and those of the other expanded, electricity will flow from the latter to the former while the condensation and expansion are going on, but as soon as two rubbing molecules are freed from each other's influence they recover their former dimensions, and the excess of the electric fluid in one of the molecules and deficiency in the other become positive and negative electric states. Non-conduction consists in a comparative deficiency of electric ether in the interval between contiguous compound molecules, by reason of which a transmission of electric movement does not readily occur from one to the other. I have shown in my paper on Molecular Physics that in such cases an electric polarization of the molecules is induced, which develops a resistance to the electro-motive force.

The excitation of electricity by heat is conceived to be principally due to the expansive action of heat on the electric envelopes of primitive molecules, and on the compound molecules, which either sets free a certain portion of electric ether, or establishes a chain of electro-polarized molecules.*

General Considerations.—The Theory of Cosmical and Molecular Physics, of which I have now given a brief outline, rests essentially upon the following principles.

(1.) *The doctrine of inertia* applicable to all matter.

(2.) *The existence of a single primary force of repulsion* exerted by every atom upon every other atom. This force is universally admitted to be in operation between the atoms of the luminiferous ether, and between the atoms of ordinary matter and this ether at the most minute distances.

(3.) *The existence of but one primary form of elementary matter*, viz: the universal or luminiferous ether:—the atoms, so called, of ordinary matter and of the electric ether being but different masses of condensed luminiferous ether. The theory of the

* For the more complete exposition of my theoretical views I must refer the reader to the memoir on Molecular Physics.

origin of universal gravitation that has been propounded might be reconciled with the ordinary notion of matter, viz: that its atoms are essentially different from those of the ether, if we could admit that the resistance of an ordinary atom to a force giving it motion was proportional to its surface instead of its mass.

(4). *The doctrine of the interception of force*, as already set forth.

(5). *The primary force of repulsion is made up of impulses recurring with an immeasurable rapidity.* This is no new hypothesis. In all treatises on Mechanics, gravity, and all incessant forces, are conceived to consist of an indefinitely great number of impulses taking effect in a finite interval of time.

If we conceive the propagation of the primary force of repulsion to be by the intervention of a medium, this medium must be an ether more subtile, and endued with a more intense elastic force than the luminiferous. This elastic force must consist in a mutual repulsion between the atoms. Thus, upon the idea of a material propagation of force, we must ultimately rest upon the conception of a force exerted between two atoms separated by a finite though excessively minute interval of space. There is no tenable position between this and that of a plenum.

Let us here devoutly acknowledge that in thus following the chain of cause and effect into the precincts of that most deeply hidden of all mysteries, the origin of force, we have come into the presence of the Infinite Spirit who puts forth unceasingly, from every point in the realms of space, His creative and sustaining power upon the subtile matter that fills all space, and is the essential substance of all worlds.

In addition to the principles just stated, we recognize the existence of matter in the three states of *the luminiferous ether*, *the electric ether*, and *ordinary matter.*

It hardly need be stated that among the consequences of these fundamental principles is included the doctrine of the *Conservation of Energy*, actual and potential. It is obvious that at any point in the boundless sea of ether the same amount of cosmical force would be received from every direction, but for the existence of the innumerable worlds dispersed through it. It is also true that in the mutual gravitation of each pair of cosmical bodies, resulting from the interception of the cosmical force, the amount of this force expended in giving motion to the one body will be expended in precisely opposite directions in giving motion to the other body; so that on the whole the cosmical force in operation in any one direction is equal in amount to that in operation in the opposite direction. Also the amount of energy taken up in the half revolution during which the one revolving body is approaching the other, is given out during

the remaining half revolution in which it is receding and returning to the original position.*

The kindred principle of the mutual convertibility, or "Correlation of Physical Forces," including among these the living force of moving bodies and of the atoms of all bodies, is obviously an essential feature of our general theory. For heat is regarded as being in its origin a wave of translation in the luminiferous ether, and an electric current is a similar wave in the electric ether, and the conversion of one of these into the other, or into the motions of ordinary matter, is merely the transfer of the living force of moving matter from one of the states or forms of matter to another. The heat developed in chemical attraction, is the amount of living force in the waves of luminiferous ether originated by the compression of the electric envelopes of the uniting molecules; and the force of attraction is expended in the act of originating these waves, and must be equal to the living force propagated in them. In the chemical union of two atoms, or primitive molecules, the "clash of atoms" that takes place cannot be succeeded by a perfectly elastic mutual repulsion, since such a repulsion would completely separate them, and restore them to their condition before the attraction came into operation. Now the loss of elasticity, which renders the union possible, consists in the outward movements imparted to a certain portion of the luminiferous ether lying between the uniting atoms and their electric envelopes, by the forcible compression of the envelopes, resulting from the clash of the atoms; and these movements constitute also the living force of heat developed.

The heat produced by extraneous pressure, or by impact, is the work done by the force of pressure in compressing the molecular envelopes, in opposition to the resistance of the

* The ever-recurring pulses of the primary cosmical force, emanating from all the atoms of the one, primary matter, are directly consumed in communicating opposite movements, or virtual movements, to every atom in the universe. It is, as I conceive, because in the existing condition of things the distribution of matter is unequal in different directions around a point, and therefore the partial interception of the impulses of the cosmical force along the different lines of direction unequal, that an effective gravitating force exists. The entire amount of the cosmical force consumed in any interval of time is the amount intercepted by all the atoms of matter, and is independent of the motions that result from the inequalities just noticed. Gravitation, and molecular and chemical attractions which originate in the gravitation of electric ether toward atoms of ordinary matter, are then derivative forces, incidental to the direct actions exerted by the cosmical force upon the atoms. The heat which escapes from the sun, or any cosmical body, into space, represents only a certain amount of the general gravitating force previously expended in imparting movements, in waves of translation, to the luminiferous ether. The electric current, wherever it exists, represents a certain amount of the same force, expended in imparting similar movements to a certain quantity of electric ether at the source of the current. In other words, the escaping heat and the electric current each represents a certain amount of work done, either by the gravitating tendency of electric ether toward ordinary atoms, or by that of ordinary atoms toward each other.

luminiferous ether posited below them, transformed into the accumulated work of the ethereal waves set in motion by this compression. The deficiency of elasticity, essential to the development of the heat, consists in this escape of the ether in waves of translation, and the attendant loss of living force; and the permanent compression of the impinging bodies results from the change in the molecular forces, consequent upon the permanent compression, or forcing inward of the molecular envelopes.

The principle of the "Correlation of Physical Forces," involves (as above implied) with it that of the "Conservation of Force," maintained by modern physicists, viz: that all transformations of one physical force into another take place without any loss, and therefore the store of living force in existence in nature is invariable.

The idea seems now to be commonly entertained that the entire force in operation in the universe is the result of certain motions imparted to all matter at the creation, and so being ever equivalent to the living force embodied in these primordial motions, must be invariable. But a gravitating force is always operating to deflect each revolving body from the tangent to its orbit, and a certain amount is continually being expended in this act, so that if the force of gravitation were due to preëstablished motions, it would be continually wasting away, and the entire amount of force in the universe would be perpetually diminishing. The sum total of the individual forces taking effect throughout all nature may indeed be constant, but if so it is because the flux of the primary force of repulsion is uniform, and is consumed primarily by interception from all atoms, incidentally in maintaining the motions of revolution of all the cosmical bodies (in the universe), and all the molecular forces perpetually neutralizing each other within these bodies.

From our present point of view we may also discern that the physical forces, ordinarily so called, have an entirely different origin from that above mentioned. We may perceive that they are all either the direct or indirect result of the operation of the general force of gravitation, which is itself a consequence of the operation of the cosmical force of repulsion. Gravitation is the direct agent in two general classes of phenomena, viz: the revolution of one cosmical body around another, and the act of condensation of every such body upon its center of gravity. In the former the motion is curvilinear, and all the living force imparted by gravity to either body, while the two are approaching each other, is taken out, by the operation of the same force in opposite directions, while they are receding from each other; and this alternation has continued from the first without loss of living force, from one revolution to another. But, in the

act of formation of the existing cosmical masses by condensation, the motions of their elementary parts have been directed toward the center of gravity of the mass, and the mutual destruction of such opposing motions has developed an equivalent amount of living force in the form of heat, that is in the form originally of waves of translation proceeding from the ethereal atmospheres of the condensed molecules. By the continued operation of the gravitating force, in the sun and earth, and probably in innumerable other worlds, the transformation of the work of condensation effected by this force into an equivalent store of heat-work is constantly going on. The heat of chemical combination, as well as that of liquefaction and solidification, are attributable to the gravitation of the electric ether toward the atoms of ordinary matter. All the heat and attendant light evolved since the creation form a store of accumulated work equivalent to the work done by the force of gravitation in effecting all the condensations that have hitherto taken place. This heat and light force has been in its natural operations opposed to the force of gravitation, and so essentially a separating and decomposing force. It has been the great physical agent in all the processes of vegetable and animal life. It has ever been passing through cycles of transformation into other physical forces, but in all its transformations the entire amount of energy, actual and potential, has remained invariably the same. Chemical combination, the electric current, liquefaction, and solidification, are instances of motion directly due to the general gravitating force operating on the electric ether. All motions of translation or rotation of bodies at the earth's surface are traceable directly to the same force, or to that of heat repulsion and therefore indirectly to that of gravitation. We may accordingly lay down the postulate, that the entire circle of what are termed physical forces have originated in the living force of the motions of condensation that have resulted from the natural operation of the general gravitating force, initiated at the dawn of creation and continued through all time. The very process of formation, by gradual condensation, of the worlds that people immensity, has developed the natural forces which have presided and continue to preside over all the processes of change that diversify and beautify their surfaces, and fit them to be the abodes of living beings. These diverse natural agents are but the offspring of the one overshadowing force, which, in the progress of countless ages has formed all worlds out of the primordial material fashioned by the Hand of the Creator. We may indeed, as we have seen, rise to a still greater height of conception, and refer this universal force to a primal force of cosmical repulsion, associated by the Infinite Spirit, the Source of all Power, with every atom of the one primeval matter that fills immensity, and is the elementary substance of all worlds.

ART. IV.—*The Photo-mapper;* by HENRY M. PARKHURST.

IT is a remarkable fact that while vision is by far the most accurate of our senses, aiding us almost exclusively in obtaining accurate measures, we have as yet discovered no method of measuring the intensity of light. The method of measuring the brightness of a star by extinction, for example, is in fact merely a method of assisting the eye in estimation. If we were to measure the diameter of a sphere by removing it to such a distance that it should cease to be visible and multiplying that distance by a certain constant; or by subdividing it until we could divide it no longer, or until the atoms should have no appreciable weight, and multiplying by a constant; it would be analogous to determining the brightness of a star by ascertaining what proportion of its light is too small to affect the retina.

The true mode of measurement is by substraction of certain known quantities. To illustrate the advantage of this, were the difference of brightness of Arcturus and Capella but .01^m, that difference would be visible to the naked eye; and were it but .0001^m, it would be easily visible with telescopic aid. Could we construct a glass which would transmit, not a certain proportion of the whole light of a star, but all its light exceeding a certain absolute quantity, it would afford us a perfect photometer. Or could we construct a glass which should transmit polarized light, but which would not transmit common light, that would accomplish the same result. But in the mean time we may be allowed to designate estimations assisted by mechanical means as measures of the magnitude of a star.

My first experiments in determining by mechanical means the magnitude of stars observed in zones, and recording that magnitude at the time of observation on star-maps taken by the instrument described in the Journal for September, were made in 1865. I then constructed a hexagonal diaphragm so arranged that a series of six plates over the object glass, moved simultaneously by a lever extending the length of the telescope, gradually diminished the aperture until the star ceased to be visible. I connected this lever with the system of levers operated by the star-key, and recorded upon the map, in the same right-ascension with each star, a point whose position indicated the aperture at the moment of extinction, and therefore gave by a prepared scale the magnitude of the star.

My next step was to avoid the long and cumbrous lever by adopting the principle of the apparatus invented by me in 1860, and which I term a Bar Photometer, reducing the cone of light from a star at a point intermediate between the object-glass and its focus, instead of directly reducing the aperture at the object-glass.

At a convenient point, say one-eighth of the distance from the focus to the object-glass, I place in the meridian a straight bar one-eighth of the diameter of the object-glass in width. Clamping the telescope a little to the west of a star, the cone of light is gradually intercepted by the bar, and gradually re-appears. The time during which the star is extinguished, or during which it remains of less brightness than a standard artificial star in the field, indicates the brightness of the star by the proportion of the light cut off by the bar at the moments of occultation and reappearance.

A modification of this plan, is to make the bar wider, so that it shall become a diaphragm, with a central aperture of the width of the cone of rays, observing the duration of apparition instead of the duration of occultation of the star.

By either of these methods the scale may be made one of equal parts by a device similar to that which will presently be explained. With or without such a device, this method is well adapted for observations of small variable stars, requiring no apparatus but the bar, whose distance from the focus shall be so determined that photometric accuracy shall not be sacrificed, while there shall be no unnecessary consumption of time. Indeed a plain Bar Photometer can at any time be extemporized in a few minutes. Its adjustment in width or distance is not essential, because its position and width can be determined and the proper connections made without difficulty after the observations.

A further modification of this plan, is to revolve the diaphragm 90°, and make it movable in declination, occultating the star on both sides of the center of the field, the magnitude being indicated by the extent of the motion, which may be conveniently and accurately measured, with a dim light, by a pair of dividers.

By a still further modification, the motion of the diaphragm may be produced, by means of intermediate levers, by the star-key of my star-mapper; and this constitutes the Photo-mapper, which I will now more particularly describe.

The diaphragm is moved parallel to itself in an arc corresponding to that described by the star-point and star-key, being supported and guided by two parallel bars whose bearings are screwed to the east side of the telescope tube. The diaphragm being seven-eighths of the distance from the object-glass to its focus, the length of the supporting bars is seven-eighths of the radius of the star-point. As I use a camera-prism, which slightly shortens the focus, I have, in my instrument, made the proper corrections, but need not here refer to details of that description.

On the same center with the star-point, and star-point-bar, shown in fig. 1 of my article on the Star-mapper, is another bar

placed parallel to the axis of the telescope, pointing toward the object-glass. The further end of this magnitude-bar is connected at right angles by a connecting-rod with universal joints, with the upright arm of a lever, the lower arm of which is the lower supporting-bar. The length of this upright arm and of the magnitude-bar must be equal. In photo-mapping I place the prism always in the meridian, to avoid the complicated adjustments which would be necessary if its position were to be varied.

As the diaphragm will move seven-eighths as fast as the star-point, it is evident that if it is so adjusted that it will not intercept any of the light of a star in the center of the field when the star-point is brought to the declination of that star, then in mapping a northern or southern star in the field its light will be unobstructed. But moving the star-key to the north or south of a star will intercept part of its rays. The light of the star being thus equalized with that of a standard artificial star, or extinguished, as the case may be, a magnitude mark is impressed on the paper. The star is then mapped in its proper position, and the distance upon the map, of the magnitude mark from the corresponding star, will be the measure of the magnitude of that star.

A perpendicular plate, with a circular hole through which the connecting-rod passes, furnishes a convenient point from which to measure with dividers when the instrument is used without the mapper.

Thus far I have spoken of the "cone" of rays, as if the aperture were circular. If it were so, the scale of magnitudes would not be one of equal parts. It may be made one of equal parts by placing over the object-glass an outer diaphragm with an aperture of suitable form, and making the aperture of the inner diaphragm of corresponding form.

Let $x=2{\cdot}5[y]$ be the equation of a logarithmic curve. Then, the area between any two ordinates of that curve will be $2{\cdot}5\mathrm{M}\triangle y$; M being Modulus. Constructing for the object-glass of 6 inches aperture a diaphragm bounded by four such curves, with values of x ranging from $-1{\cdot}5$ inches to $+1{\cdot}5$ inches, laying off x vertically, above and below the center, and y to the right and left, and a similar inner diaphragm of one-eighth the lineal dimensions, neglecting for the present the portion of the curve which will pass outside of the circle, it will be seen that the motion of each one-fourth of an inch of the inner diaphragm will leave an area corresponding to stars exactly one magnitude smaller. The motion of the star-point will be one-seventh greater, and of the star-key still greater, according to the scale of the map.

But while the object-glass is limited, the inner aperture may

extend beyond the corresponding circle of one-eighth the diameter. If therefore an area equal to twice the omitted portion of the curves above and below is added to the inner diaphragm, it will be almost immediately available, and the omission from the outer aperture will cause an error of less than $\cdot 1^m$, and that only at the commencement of the scale. The proper form of this added portion is the inversion of the last $1^m 2$ of the curve within the circle, the final value of y being exactly three times its value at the margin of the disk.

With the diaphragms thus constructed, the scale runs through $3^m 0$ as a scale of equal parts. Beyond that point the star may still be extinguished, if not too near the center of the field; but the scale will be condensed.

The stars occurring in any zone may be divided into three classes: those above the $6^m 5$ being approximately measured by the condensed scale, and being so few in number that they may be conveniently, as well as more accurately, measured by a different method, to be explained below; those between the $6^m 5$ and the $9^m 5$, which may be equalized in brightness with an artificial star in the field previously brought to an equality with a $9^m 5$ star; and those fainter than the $9^m 5$, which may be extinguished. The outer diaphragm will so diminish the aperture that $12^m 5$ stars will be the smallest which can be seen.

The Disk Photometer.—For the brighter stars I employ an entirely different apparatus, based upon a more accurate method. I expand the stars into disks by drawing out the eye-piece beyond the focus, until a portion of the disk shining through an aperture in the field of view shall either be exactly equal in brightness with an adjacent luminous disk, or be imperceptible. The extinction of a disk by expansion is only practicable with a small aperture or with telescopic stars, and is not much, if any, more accurate than extinction by reducing the aperture. But comparison of disks, especially for the stars visible to the naked eye, is the most accurate means of measurement known to me, the error of the comparison being in my experience less than the inequalities of the artificial disk, and the latter much less than those of the sky.

The star and the disk can be compared, if desired, by the aid of polarization, by the use of a double-refracting prism to combine the two disks into one, and a selenite plate and another double-refracting prism to form new colored disks; but instead of producing the final adjustment by further polarization, the bundle of plates may be dispensed with, and the disks rendered colorless by the motion of the eye-piece.

For convenience I have made a slide with six equal openings, to shade the disk with neutral shades, and a slide to shade the star. I ascertained that one shade was equivalent to $1^m 03$,

and that, although I admit the rule would not be accurate with colored shades, five neutral shades were equivalent to $5^{m}15$. It is therefore convenient to compare directly stars differing as much as α and λ Ursæ Minoris. In order to neutralize the yellow color of the artificial disk, and to make it appear indistinguishable from the visible portion of the disk of a star, I have found it necessary to use a blue shade. I have extended the telescope by various tubes, sometimes nearly two feet; but with the use of the slides I find that a tube five inches long to hold the eye-piece, usually gives sufficient extension to the telescope and sufficient range of brightness. The amount of the extension I measure with dividers, using a prepared scale which gives directly, in magnitudes, the difference in brightness between the observed star and a standard depending on the brightness of the disk.

The disk photometer is especially useful for observing the brighter variable stars. It is inapplicable for determining the brightness of stars so close together that the space between them is not sufficient for a disk; but in this case it may be used to determine the aggregate brightness of the two stars. It may be used for measuring the brightness of planets, small nebulæ and comets, probably of the moon, and possibly of the sun.

The same apparatus may be used to determine the comparative brightness of different portions of the sky, but requires a different process.

The error of the determination of the magnitude of a star by the method of equalizing disks, may be divided into four parts:

I. The error of the assumed magnitude of comparison stars. In my series of observations, instituted to determine the relative amount of the several errors, this error hardly exceeds $^{m}\cdot01$.

II. The error of observation, or inaccuracy of judgment as to the exact point where the disks are equalized. Not only according to the statements of Arago, Silliman, and Crookes, reduced by myself, but according to my own results, the error of observation—I refer to the *mean* error—does not exceed $^{m}\cdot02$, in a series of stellar observations averaging forty to the hour. For fainter stars than I observed in my series, the error of observation is greater. There are three limitations:

1. One disk cannot be distinguished in brightness from another, unless one-sixtieth brighter or fainter, according to Arago.

2. One disk cannot be distinguished as brighter than another, if the subtraction of the light of the fainter disk from the other would leave a disk too faint to be visible by itself. The same principle is applicable to points of light. Unless, therefore, the disk is at least sixty times as bright as is necessary for visibility, the error of observation will be greater.

3. If the disk is small, the accuracy of comparison is impaired. Hence points of light cannot be as accurately compared as disks. I have used a disk of an apparent magnitude of at least 1°, and have not investigated the amount of error with a smaller disk.

III. The error from the variation of the artificial disk during observation. I have made many and careful experiments to render this error as small as possible, and to avoid ascribing to the irregularity of the sky discrepancies which might be owing to variations of the artificial disk. The brightness of the artificial disk may vary from four causes:

1. The illuminating quality of the gas may vary. Hence observations on different evenings cannot be compared directly. But the change of the gas will be so slow, excepting probably from the heating of the apparatus when it is first lighted, as not to appreciably affect the results.

2. From variation of the pressure the flame may be made larger and brighter. I first partially corrected this by forming upon the screen an image of a circle of the flame ·05 in. in diameter. I afterwards made a gas regulator, admitting the gas into an inverted receiver suspended at one end of a lever, the other end of which gradually shut off the gas as the receiver rose; so that whatever the pressure of gas in the mains there should be no variation in the flame.

3. The flame varies in different parts. Hence, if on turning the telescope upon a new star the lantern is not accurately adjusted to a horizontal position, a brighter or fainter portion of the flame will come opposite the aperture. The flame as a whole having been made uniform by the regulator, I have introduced a smoothly ground glass at the aperture nearest the flame, and reduced the flame and moved it further back, so as to use the whole flame.

4. The light from the star is a sharp cone, wholly entering the eye even if withdrawn several inches from the eye-piece. The light from the illuminated screen is a hemisphere. Unless therefore the eye-piece renders the rays parallel, which it will not if the observer is either near-sighted or far-sighted, a difference in the distance of the eye will greatly affect the results. I have therefore turned the plane glass in the eye-piece so as to throw the light directly into the eye, reducing the aperture of the lenses so that the cone of rays from the artificial disk nearly corresponds with that from the star. It is necessary that the eye should be held so that both cones shall completely enter it at once, which can be easily accomplished but requires care.

The error from the variation of the artificial disc, without the gas regulator, I ascertained not to exceed ^{m}·04. How much this has been reduced by introducing the regulator and ground glass screen, I have no means of estimating.

IV. After allowing for the previous errors, amounting in the aggregate to less than $^{m}\cdot05$, the remaining discrepancies of observation of invariable stars must arise from variations of the apparent brightness of the stars from atmospheric obscuration. I divide this into two parts:

1. The general and permanent obscuration depending on the altitude of the star. I have ascertained that this follows the law of refraction; but the coefficient varies in amount on different evenings, sometimes equalling $^{m}\cdot50$ for an altitude of 20°, and at other times not exceeding one-third that amount. I apply the correction for obscuration to each star, in reducing the observations, so as to eliminate this; and I therefore regard it as no part of the error of observation. The correction may be easily applied without directly determining the altitudes, by a prepared scale.

2. The temporary and local obscuration, from various atmospheric causes. By more than 400 observations of 18 stars of the 2nd to the 4th magnitude, α Persei and γ Arietis being the extremes, I found that the average mean error of an observation, from all causes, amounted to $^{m}\cdot11$; so that the mean error from the temporary and local obscuration alone is on the average $^{m}\cdot10$. There is a great difference between different days; the average mean error of all the stars being sometimes as low as $^{m}\cdot07$ for a whole evening, and at other times for an evening apparently equally clear, as high as $^{m}\cdot20$. The difference between the stars is no less conspicuous. The stars ϵ and γ Persei are near each other, and nearly of the same brightness. Yet the mean error of an observation of γ Persei, assuming it to be an invariable star, is nearly three times as great as of ϵ Persei.

It is evident therefore that if the results I have reached are even approximately correct, the errors of observation and of the instrument, if ordinary care is used, are of no material consequence; and accurate results can only be obtained by multiplying observations on different evenings, so as to eliminate as far as possible the errors arising from the variations of the sky.

ART. V.—*On the Crater of Haleakala, Island of Maui, Hawaiian Group;* by Prof. W. D. ALEXANDER. (From a letter to one of the Editors.)

I HAVE just been spending a summer vacation on Maui, and in the course of it made a careful survey of the great crater of Haleakala. During the vacation I went three times to the summit. The first time I rode up from Makawao before sunrise, and spent about seven hours in collecting mineral specimens and

plants, and forming a plan for the survey of the crater. The flora of this region has been described before. I will only say that the principal plants that survive on the bleak summit are the *Argyroxiphium*, *Raillardia montana*, *Vittadinia*, and some stunted ferns, viz: *Pteris aquilina* and *Trichomanes*. In the belt between one and four miles from the summit, in addition to the above, are found the beautiful *Geranium cuneatum*, *Sophora chrysophylla*, Sandalwood, *Coprosma*, and a few ohelos (*Vaccinium reticulatum*). Water boiled at 193° Fahrenheit when the atmosphere was at 46° Fahrenheit. At sunrise we enjoyed the grand sight of the vast triangular shadow of the mountain projected on the clouds in the western sky.

On the morning of August 4th, I ascended the mountain again from Makawao, with five natives, and furnished with a superior theodolite, a dozen large bamboos for signal poles, a good tent, and provisions for a week. We spent seven days on the mountain, and enjoyed almost uninterrupted fine weather.

We commenced operations by setting signals on the prominent points along the western side of the crater. The western cliffs are very steep, though it is possible to descend in many places, and are from 2,000 to 2,500 feet high. We pitched our tent on the lee side of a hill near the southwest corner, called by the natives "Pakaoao" or the "fortress of Kaoao." This hill is composed of a light gray solid clinkstone, which splits into laminæ like slate.

It has been much shattered, probably by the terrible convulsions that attended the opening of the Koolau gap, and for a quarter of a mile toward the northwest the ground is strewed with fragments of rock that have been hurled in that direction. These rocks form a striking contrast with the darker and more basaltic rock to the northward. The same formation crops out on the east side of the Koolau gap, at the southern foot of Hanakauhi in the oasis surrounded by recent lava, at which place it projects from the hill side in the form of huge perpendicular slabs or lamellar masses, not more than ten feet thick, and from thirty to fifty feet high, standing out of the grassy slope like immense grave stones. The patches of gravel on the summit contain numerous crystals of augite. The hill called the fortress of "Kaoao" is partially terraced on the lee side, and is covered with hundreds of little inclosures built of stone, three or four feet high, and paved with thin flat pieces of clinkstone. I noticed that a few had been covered over with a kind of slate roof. Here, according to tradition, encamped the army of Kaao, a chieftain who had been driven out of Kaupo by his rival, some time in the early part of the last century. We found a couple of ancient sling stones in the camp. Many silver-swords (*Argyroxiphium*) and other plants have grown up,

flourished and died in the *pas* where his army once encamped. I was told that they drew their supplies from *Kula*, the district on the west side. Other natives relate that the sandalwood cutters used to encamp there forty or fifty years ago, when collecting sandal wood for their chiefs. The chief, Kaao, was finally defeated and killed in a battle fought on the west side of the mountain.

Our supply of water was brought from a water-hole two or three miles west of *Kalahaku*, which is only known by a few of the natives. We spent three nights on the spot.

About a quarter of a mile farther south is a red scoria hill, which is the highest point of the mountain, 2,750 feet above the bottom of the crater. It is the first of a long line of scoria hills which extends to the sea on the southwest, passing through Capt. Makee's plantation, and is called *Kolekole*.

The true meridian was obtained by careful observations on the pole star when on the meridian taken at this point and also at the east end of the base line in the crater. The fact had been noticed before that there is a considerable local attraction in the walls of this crater. I found that certain rocks of a compact bluish lava on the highest point were decidedly magnetic. Pieces of this rock have distinct polarity. There was evidently a large daily variation of the needle; between sunrise and noon the needle moved at least half a degree westward, and returned to its former position at night. On the morning of the 5th there was a heavy frost around our camp, the thermometer at sunrise standing at 38° Fahr. The average temperature at noon was 72° in the shade; the temperature in the crater averaged about 4° higher. After selecting stations and setting up signal flags along the western and southern sides of the crater, we descended into it at the southwest corner, along an immense sloping bank of reddish sand and scoria. We then marked out a base line more than a mile and a half long over a level plain at the foot of the southern wall of the crater.

This plain was sprinkled with myriads of silver-swords (*Argyroxiphium*), many of which were in full bloom, presenting a magnificent sight. These plants are most abundant over the volcanic sand in the central part of the crater. Both in descending into the crater and on our return, we followed a beaten path, which leads through the southeast gap to Kaupo. Our party were somewhat affected by the rarity of the air; we all experienced shortness of breadth, and some complained of headache, but after spending a week in this region, we became accustomed to the air, and our "wind" improved wonderfully.

The natives have many local names for different parts of the crater. The term *Haleakala* properly belongs to the southeastern part of the mountain, extending two miles west from the

Kaupo gap to the low place in the wall of the crater, called the "Puali" or pass of "*Koanui o Kane.*"

On the eastern side is a famous rock called *Pohaku Palaha*, which is the "hub" of East Maui, from which all the boundaries between lands are believed to radiate.

The next day, the 6th, we moved our tent and baggage four or five miles into the crater, and encamped near a cave called "*Ke ana ma ka uahi*," i, e, the smoky cave, half a mile from a trickling stream on the southern wall called "the water of Palaoa." This place we made our head-quarters for three days, during which we enjoyed almost cloudless weather, and our survey made rapid progress.

There is much more vegetation in the eastern end of the crater than in the western. On the sloping walls of this part of the crater, and on the three easternmost cones we noticed a good many mountain plants, such as *Dodonæa*, *Sophora*, *Raillardia* and *Geranium*, and on the summit of the southeastern walls of the crater, we observed clumps of large trees. This vegetation gave this region a very different aspect from the stern desolation of the western end. Near the commencement of the Kaupo gap, close to the road, there is a famous water-hole called "*Ka wai pani.*" Here, as I am told, there is a subterranean stream of pure cold water, two or three feet below the surface, which is reached through a small hole in the rock, which was formerly kept closed by a large stone, whence its name, "the closed water."

The scenery around us was entirely unique, and as grand in its way as that of the Yosemite valley. The vast plain surrounded by black precipices nearly twenty miles in circumference, and from 1,000 to 2,500 feet in height, with the two wide breaks through which we had glimpses of the distant ocean, the sixteen crater cones, of different colors, gray, red and black, the profound solitude and silence broken only by the flocks of wild goats, formed a scene to which it would not be easy to find a parallel elsewhere in the world. I ascended several of these cones in the course of my triangulation, and found the highest to be over 750 feet in height above its base. Another measured 390 feet, and several are over 200 feet in height. They are composed of light scoria and cinders, generally of a deep reddish color, but two are of a peculiar gray tint, and others nearly black. I found no traces of sulphur and very little pumice. The cones all have their ancient names, which are known only by the mountaineers and old natives. For instance, the highest cone is called "*Ka lua o ka Oo*," the crater of the Oo, and was the former residence of Kamohoalii, Pele's younger brother, "the king of vapor," of whom many

legends are told. Another was called "*Ka iwi o Pele*," Pele's bone, and close by it on the north side is the *Pa puaa o Pele*, or "Pele's pig-pen." This is a singular natural formation of lava, about two rods square and ten feet high, with an opening on the northwest side. Some of my men believed that if any one should *kaha*, i. e., scratch the sand in this place or otherwise desecrate the sacred precinct, the evil spirits of the mountain would bring on fog and rain. A similar superstition attaches to the *Argyroxiphium*, or silver-sword plant. The natives also report a bottomless hole on the northwest part of the crater, which I did not visit, but which has been seen by my father and others.

After returning to our first camp on the summit, we spent another day in visiting the stations around the south and west sides, and measuring the remaining angles, and returned to Makawao late in the evening of the 11th. After having worked up my notes and drawn a map of the crater, I visited the summit once more on the 25th, to compare my map with the original on the spot, and to make the details more exact.

According to the results of my survey, the crater is of an oval shape, its longest diameter being nearly east and west, unlike most of the Hawaiian volcanoes, the major axes of which generally range north and south. The greatest length of the crater was found to be about seven and a half miles from east to west, the width being from two and a quarter to three miles. The area is about sixteen square miles.

The highest point was determined by the United States Exploring Expedition to be 10,217 feet above the level of the sea, and 2,783 feet above the bottom of the crater. I made its height by triangulation 2,750 feet above the east end of my base line in the crater. The boiling water experiment gave an altitude of 10,165 by Regnault's rule, which is certainly a close approximation.

The northern or Koolau break is about three miles wide, and over 2,000 feet deep, where it first leaves the crater. The western brink of it bears about N. 64° E. The bottom of it is floored with streams of lava of different ages, and is extremely rugged.

The southeast or Kaupo break is about a mile and a quarter wide, and its direction for the first three or four miles is about S. 58° E. The lava flow then turns to the southward, and continues in the direction S. 34° E. to the sea, spreading out in the form of a delta, and filling up the lower part of several large ravines.

The appearance of the lava fields and cones in the eastern end, and the vegetation on them seem to indicate that they are far more ancient than those in the western end. The latest

action in the crater seems to have been the formation of the cinder cones in the southwestern part.

To conclude, the survey has impressed me with the conviction that this is a real terminal crater, and not merely "a deep gorge open at the north and east," or a *caldera*. I have indeed heard the theory proposed that the mountain is but the wreck of a complete dome with a small terminal crater, the whole top of which has fallen in and been carried away, as is supposed to have been the case with some of the volcanoes of Java, and the Caldera of Palma. Such questions I leave for geologists to settle, and if I can furnish them any new data on the subject, I shall be quite content.

Oahu College, Oct. 12, 1869.

ART. VI.—*On a new method of separating Tin from Arsenic, Antimony, and Molybdenum;* by FRANK WIGGLESWORTH CLARKE, S.B.

SOME time since, happening to notice that the remarkable highly crystalline precipitate formed by oxalic acid in a solution of stannous chlorid was not blackened or otherwise affected by sulphuretted hydrogen, I was led to a series of experiments concerning the action of the above named acid upon certain metallic sulphids, and obtained the following results.

Both sulphids of tin, if moist, and freshly precipitated, are readily decomposed by moderately long boiling with an excess of oxalic acid, H_2S being given off. The monosulphid is converted into the insoluble, crystalline stannous oxalate, while the yellow disulphid is completely dissolved. The commercial "mosaic gold," however, seems to be unacted upon by the reagent. In presence of an excess of oxalic acid, tin cannot be precipitated by H_2S.

The sulphids of arsenic, even upon very long boiling with the acid, are almost unattacked. Very minute traces of the metal sometimes go into solution, but may be reprecipitated by a bubble or two of H_2S. Accordingly, the presence even of an enormous excess of oxalic acid does not hinder the precipitation of arsenic as sulphid.

The sulphid of antimony behaves in a somewhat different manner. Although upon long boiling with oxalic acid considable quantities of the metal are taken into solution, yet every trace of it may be reprecipitated by H_2S.

Molybdic trisulphid appears to be wholly unattacked by oxalic acid, even upon very long boiling.

With the sulphids of tungsten I have obtained discordant results. Under certain circumstances they seem to be wholly

insoluble in the acid, while at other times they are decomposed completely, and partly taken into solution.

By availing myself of the solubility of the sulphids of tin in oxalic acid, I have been enabled to separate tin perfectly from arsenic and molydenum, and almost perfectly from antimony. When only arsenic and antimony are to be separated from tin, I find it best to proceed as follows: to the solution containing the three metals, this solution being prepared in the usual manner for the precipitation of the sulphids, I add oxalic acid in the proportion of about twenty grams of the reagent for every gram of tin, taking care to have the whole so concentrated that the acid will crystallize out in the cold. I then heat to boiling, and pass in sulphuretted hydrogen for about twenty minutes. No precipitate appears at first, but as soon as the liquid is saturated with the gas, the sulphids of arsenic and antimony begin to fall, and in a very few moments are completely thrown down. Then, as usual, the whole should be allowed to stand about half an hour in a warm place before filtering. Every trace of arsenic and antimony is precipitated, so that in the filtrate from the sulphids neither of these metals can be discovered by Marsh's test, nor can any antimony stain be produced with zinc upon platinum. I have carefully experimented to learn whether oxalic acid could interfere with either of these tests, and find that it has not the slightest influence upon them. The sulphid of arsenic is absolutely free from tin, but the antimony always carries down a minute trace of that metal with it. This trace, however, if the operation has been carefully performed, can scarcely be detected, and generally may be ignored with safety. If, however, the greatest accuracy is desired, it may be well to redissolve the sulphid of antimony in an alkaline sulphid, decompose the solution with an excess of oxalic acid, boil with a little strong sulphydric acid water, filter, and add the filtrate to the tin solution previously obtained.

To separate tin from molybdenum, owing to the difficulty of precipitating the latter metal with H_2S, I have been obliged to slightly vary my process. I find that by adding an alkaline sulphid in excess to a solution containing a molybdate, then decomposing the sulphur salt formed with a considerable quantity of dilute chlorhydric acid, and allowing the whole to stand over night in a warm place, every trace of molybdenum is precipitated. The sulphid thus obtained can be easily washed with a mixture of dilute chlorhydric acid and ammonic chlorid. If now, by this process we throw down tin and molybdenum together, every trace of the former metal may be dissolved out by boiling the mixed sulphids for about three fourths of an hour with oxalic acid in the proportions which I have already given.

It is best to have present in the solution, while boiling, a little dilute chlorhydric acid.

If antimony also is contained in the mixture, it is necessary, just before ceasing to boil, to add to the solution an equal volume of strong sulphydric acid water, to reprecipitate any of that metal which may have gone into solution. Upon filtering no molybdenum can be detected in the filtrate by any ordinary tests, and the molybdic sulphid is absolutely free from tin. In all these cases it is assumed that the tin is in the form of a stannic compound. It must be borne in mind that the lower sulphid of this metal is converted by the acid into an oxalate insoluble in water. But as the latter dissolves to an almost unlimited extent in dilute HCl, its formation need not interfere with an analysis.

Since the presence of oxalic acid interferes somewhat with the complete precipitation of tin by ordinary methods, I was subjected to some trouble in finding a process for determining that metal after the separation. At last I found it could be thrown down as follows; the solution, after being rendered slightly alkaline with ammonia, is mixed with enough ammonic sulphid to redissolve the precipitate at first formed, an excess of *acetic* acid is added, and the whole allowed to rest several hours in a warm place. *Acetic* acid must be used, for stronger acids would be liable to set free some of the oxalic to redissolve the tin. The precipitate which at first varies from white to pale yellow, rapidly darkens in color, and seemingly consists of a mixture of oxyd and sulphid of tin. It should be washed with a solution of ammonic nitrate, and, after ignition, is weighed as SnO_2. In two successive experiments, in which I mixed a weighed quantity of tin with unknown proportions of arsenic and antimony, I received of the tin, after making my separation, respectively 99·93 and 99·57 per cents. The loss in the second case was due to my not having allowed the tin precipitate to settle sufficiently long before filtering, in other words, to incomplete precipitation.

The arsenic and antimony, being in the form of sulphids, may be estimated by any of the ordinary methods. They may be best separated by Bunsen's process with sulphurous acid, which, though far from perfect, is superior to all others. Lenssen's method, in which the arsenic is precipitated from the sulphur solution of the two metals as ammonia-magnesian arsenate, is worthless.

A couple of years ago I made a few experiments upon indirectly determining the proportions of tin and antimony in alloys of the two metals. I oxydized a weighed quantity of the alloy with nitric acid in a porcelain crucible, heated the resulting oxyds with ammonic nitrate, and then, regarding the

tin as converted into SnO_2 and the antimony into Sb_2O_4, calculated the proportions of the metals from the increase in weight. This method, although by no means giving me accurate results, served very well for rough approximate determinations. I cite it here simply as an easy and convenient process for obtaining a close idea of the constitution of any alloy composed of the two metals. Possibly the method might be so modified as to give accurate determinations.

ART. VII.—*Notes on the structure of the Crinoidea, Cystidea, and Blastoidea;* by E. BILLINGS, F.G.S., Palæontologist of the Geological Survey of Canada.

[Continued from this Journal, II, vol. xlviii, p. 83.]

5. *On the homologies of the respiratory organs of the Palæozoic and recent Echinoderms, and on the "Convoluted Plate" of the Crinoidea.*

IN a former note I have advanced the opinion that:—"The grooves on the ventral disc of *Cyathocrinus* and, also, the internal "*convoluted plate*" of the Palæozoic Crinoids, with the tubes radiating therefrom, belong to the respiratory and, perhaps, in part, to the circulatory systems—not to the digestive system. The convoluted plate with its thickened border seems to foreshadow the "œsophageal circular canal" with a pendant madreporic apparatus as in the Holothuridea." (This Journal, II, vol. xlviii, p. 73). I should have referred it to the madreporic system of the existing Echinodermata in general, instead of to that of the Holothuridea in particular. At the time the note was written I had in view the madreporic sack of *Holothuria* which, as will be shown further on, most resembles in form that of *Actinocrinus*. The figures and descriptions, which follow, are intended to show the gradual passage or conversion of the respiratory organs of the *Cystidea*, *Blastoidea* and *Palæocrinoidea* into the ambulacral canal system of the recent echinoderms, and that as the convoluted plates of the former have the same structure and connections as the madreporic sacks and tubes or sand canals of the latter, they are, most probably, all the homologues of each other.

Among the Cystideans we find several genera, such as *Cryptocrinites*, *Malocystites*, *Trochocystites*, and apparently some others, whose test is totally destitute of respiratory pores, being composed of simple, solid plates like those of the ordinary Crinoidea. In a second group of genera, among which may be enumerated *Caryocystites*, *Echinosphærites*, *Palæocystites and Protocystites*,

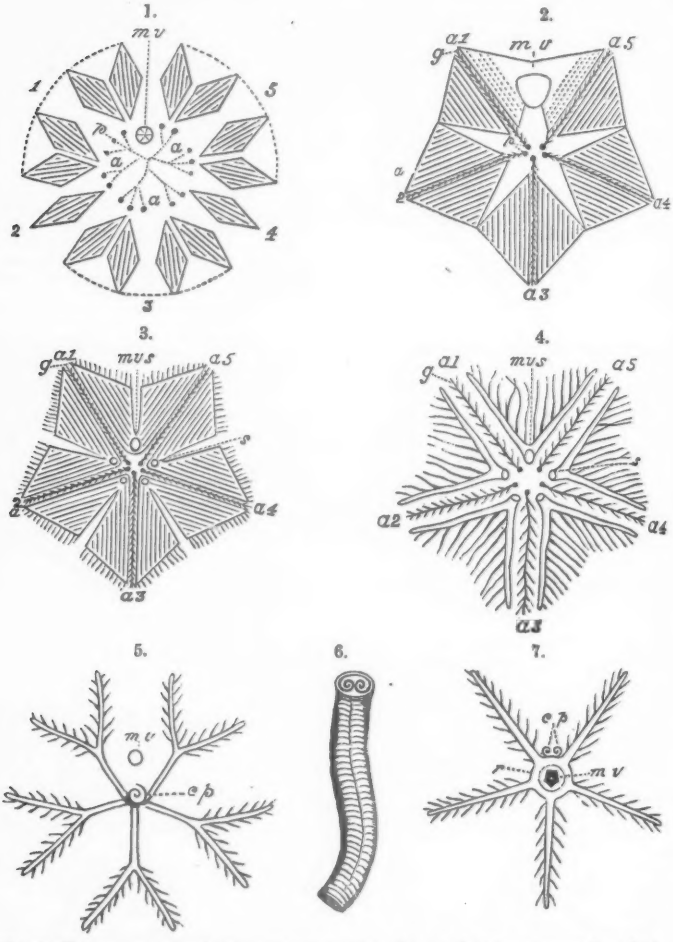


Fig. 1. The upper part of *Caryocrinus ornatus*, the test being removed in order to show the internal structure of the fourteen hydrospires that surround the summit. The parallel lines represent the flat tubes. The other figures exhibit the modifications which the hydrospires undergo in passing through:— 2. *Codaster*. 3. *Pentremites* with broad ambulacra. 4. *Pentremites* with single tubes. 5. *Palæozoic Crinoids* with a convoluted plate attached to the centre of radiation. 6. Sand canal or madreporic tube of a starfish inclosing a doubly convoluted plate. 7. Ambulacral canals of a starfish with the doubly convoluted plate of the sand canal attached to the œsophageal ring. The following letters have the same reference in all the figures in which they occur: *a*, an arm or ambulacrum; *m v*, mouth and vent combined in a single aperture; *m v s*, mouth, vent and spiracle; *g*, ambulacral groove; *p*, ovarian pore; *s*, spiracle; *c p*, convoluted plate; *r*, œsophageal ring.

the whole of the external integument seems to have been respiratory, as all, or nearly all, of the plates of which it is composed, are more or less occupied by variously arranged, poriferous or tubular structures. The Cystideans of these two groups hold the lowest rank of all those known. In their general structure they are mere sacks of a globular, ovate or, (as in the case of *Trochocystites*) flattened form. Their test consists of an indefinite number of plates without any radiated arrangement. They were also, according to our present knowledge, the first to make their appearance, two of the genera, *Trochocystites* and *Eocystites*, having been discovered in the primordial zone. No other echinoderms have been found in rocks of so ancient a date.

Next in order may be placed those genera whose test is composed of a definite number of plates, which have, to some extent, a quinary arrangement. Thus, *Glyptocystites*, *Echinoencrinites*, *Apiocystites* and several others, have each four series of calycine plates, of which there are four plates in the basal and five in each of the other three series. The respiratory areas or hydrospires are reduced in number—ten to thirteen in *Glyptocystites* and three in most of the other genera of the group. Neither in the plates nor in the hydrospires is there exhibited any tendency to a radiated arrangement. The most ancient genus of this family is *Glyptocystites*, which first appears in the Chazy limestone and seems to have become extinct in the Trenton. The other genera occur in various horizons between the Chazy and the Devonian.

In the genera *Hemicosmites* and *Caryocrinus* the hydrospires in the upper part of the test converge toward, but do not reach, the central point of the apex, thus forming the commencement of that concentration and complete radiation which is exhibited in the ambulacral canal system of the higher echinoderms. In a former note (this Jour., II, xlviii, p. 77,) it is pointed out that *Caryocrinus* has thirty hydrospires,—ten at the base with their longer diagonals vertical,—a zone of six round the middle with their diagonals horizontal and a third band of fourteen around the upper part of the fossil. These latter are represented in fig. 1, as if spread out on a plane surface. On consulting this figure it will be seen that the flat tubes of the hydrospires, represented by the parallel lines, all converge toward the central point from which the dotted lines radiate. This point is the position of the mouth in the recent echinoderms, but in *Caryocrinus* it is occupied by a large solid imperforate plate. The hydrospires are arranged in five groups. Commencing at *m v* and going round by 1, 2, &c., there are four in the first group; one in the second; four in the third; one in the fourth and four in the fifth. These five groups represent the five ambulacral canals of the recent echinoderms. In the specimen from which this dia-

gram was constructed there are the bases of fifteen free arms to be seen situated at the outer extremities of the dotted lines. At the base of each arm there is a small pore, *p*, which I believe to have been exclusively ovarian in its function. The hydrospires have no connection whatever with the arms and are, moreover, all of them entirely separated from each other. If then they represent the ambulacral system of the recent echinoderms, it is quite certain that that system was at first, (or in the undeveloped stage in which it existed in the Cystidea,) destitute of the œsophageal ring.

In *Codaster* a further concentration of the respiratory organs is exhibited. There are here only five hydrospires and they are all confined to the circle around the apex. Two of them are incomplete in order to make room for the large mouth and vent (*m v*, fig. 2.) They are each divided into two halves by an arm, *a*1, *a*2, &c. They are only connected with the arms to this extent, that these latter lie back upon them. The arms are provided with pinnulæ but it is not at all certain that they (the pinnulæ) were in any direct communication with the hydrospires. It is evident that *in all the Cystidea*, (and in none is it more obvious than in *Caryocrinus*), there was no connection between the hydrospires and the pinnulæ. The main difference (so far as regards the evidence of the presence or absence of such a connection) between *Caryocrinus* and *Codaster*, consists in this, that in the former the arms are erect and do not touch the hydrospires, whereas in the latter they are recumbent and lie back upon them. Each of the arms of *Codaster* has a fine ambulacral groove and all of the grooves terminate in a single central aperture. But as this aperture was covered over by a thin plated integument, as in the Blastoidea, I have not shown it in the diagram, but only the five pores, *p*.

No one who compares a *Codaster* with a *Pentremites* (the internal structure of the latter being visible) can doubt that the hydrospires of the two genera are perfectly homologous organs. If we grind off the test of a species of the latter genus, selecting one for the purpose which has broad petaloid ambulacra such as those of *P. Schultzii*, the structure exposed will be that represented in the diagram, fig. 3. In *Pentremites* as in *Codaster*, the five hydrospires are divided into ten equal parts by the five rays, *a*1, *a*2, &c. In *Codaster* these ten parts remain entirely separate from each other, but in *Pentremites* they are re-united in pairs, the two in each interradial space, being so connected, at their inner angles, that their internal cavities open out to the exterior through a single orifice or spiracle (*s*, figs. 3 and 4). This is best shown in fig. 4, intended to represent the structure of *P. ellepticus* (Sowerby) as described by Mr. Rofe, Geol. Mag., vol. ii, p. 249. In this species the hydrospires instead of being

formed of broad sacks, with a number of folds on one side, consist of ten simple cylindrical tubes connected together in five pairs. The only difference between the structure of fig. 3 and fig. 4 is in the width of the tubes and in the absence of folds in the latter. These two forms are moreover connected by intermediate grades. Species with 11, 10, 8, 6, 5, 4 and 2 folds being known, there is thus established a gradual transition from the broad petaloid form to the single cylindrical tube.

Between the *Cystidea* and the *Blastoidea* the most important changes are, that in the latter the hydrospires become connected in pairs, and, also, are brought into direct communication with the pinnulæ. In the Palæozoic Crinoidea (or at least in many of them) concentration is carried one step further forward, the five pairs of hydrospires being here all connected together at the centre as in fig. 5. There is as yet no œsophageal ring, (as I understand it) but in its place the convoluted plate described in the excellent papers of Messrs. Meek and Worthen This organ, according to the authors, consists of a convoluted plate, resembling in form the shell of a *Bulla* or *Scaphander*. It is situated within the body of the Crinoid with its longer axis vertical and the upper end just under the centre of the ventral disc. Its lower extremity approaches but does not quite touch the bottom of the visceral cavity. Its walls are composed of minute polygonal plates or of an extremely delicate network of anastomosing fibres. The five ambulacral canals are attached to the upper extremity, radiate outward to the walls of the cup and are seen to pass through the ambulacral orifices outward into the grooves of the arms. (Ante, vol. xlviii, p. 31.)

The ambulacral canals of the Crinoidea are, for the greater part, respiratory in their function. They are, however, as most naturalists who have studied their structure will admit, truly the homologues of those of the Echinodermata in general. In the higher orders of this class the canals are usually more specialized than they are in the lower; being provided with prehensive or locomotive organs. In all of the existing orders, including the recent Crinoidea, we find an œsophageal ring.

To this organ, which is only a continuation of the canals, are attached the madreporic appendages. These consist of small sacks or slender tubes varying greatly in form and number in the different genera. That of the Starfish *Asteracanthion rubens* is thus described by Prof. E. Forbes. "On the dorsal surface is seen a wart-like striated body placed laterally between two of the rays: this is the *madreporiform tubercle or nucleus.* When the animal is cut open, there is seen a curved calcareous column running obliquely from the tubercle to the plates surrounding the mouth; Dr. Sharpey says it opens by a narrow orifice into the circular vessel. It is connected by a membrane

with one side of the animal, and is itself invested with a pretty strong skin, which is covered with vibratile cilia. Its form is that of a plate rolled in at the margins till they meet. It feels gritty as if full of sand. When we examine it with the microscope we find it to consist of minute calcareous plates, which are united into plates or joints, so that when the investing membrane is removed it has the appearance of a jointed column. Professor Ehrenberg remarked the former structure, Dr. Sharpey the latter: they are both right. Both structures may be seen in the column of the common cross-fish." (Forbes, British Starfishes, p. 73.)

In Prof. Joh. Müller's work, "Uber den bau der Echinodermen," several forms of the madreporic appendages of the different groups of the recent Echinodermata are described. In general they are composed of a soft or moderately hard skin consisting of a minute tissue of calcareous fibres, or of small polygonal plates. The walls are also, sometimes, minutely poriferous. In all the Holothurians the madreporic organ is a sack attached by one of its ends to the œsophageal canal, the other extremity hanging freely down into the perivisceral cavity, not connected with the opposite body wall as is the sand canal of the starfishes. (Op. cit., p. 84.) In its consisting of a convoluted plate the madreporic organ of *Actinocrinus*, therefore, agrees with that of the starfishes, while in its being only attached at one extremity it resembles that of the Holothurians.

The convoluted plate of the Palæozoic Crinoids and the madreporic sacks and tubes (or sand canals) of the recent Echinoderms, therefore, all agree in the following respects:—

1. They have the same general structure.
2. They are all appendages of the ambulacral system.
3. They are all attached to the same part of the system, that is to say, to the central point from which the canals radiate.

The above seems to me sufficient to make out at least a good *prima facie* case for the position I have assumed. When among the petrified remains of an extinct animal, we find an organ which has the same general form and structure, as has one that occurs in an existing species of the same zoological group, we may, with much probability of being correct in our opinion, conclude that the two are homologous, even although we may not be able positively to see how that of the fossil is connected with any other part. But when, as in this instance, we can actually see that it is an appendage of another organ, or system of organs rather, which is known to be the homologue of the part with which that of the existing species is always correlated, we have evidence of a very high order on which to ground a conclusion. By no other mode of reasoning can we prove that

the column of an *Actinocrinus* is the homologue of that of *Pentacrinus caput Medusæ*.

In an important paper entitled "Remarks on the Blastoidea, with descriptions of New Species" which Meek and Worthen have kindly sent me, the authors, in their comments upon my views, state that:—

"In regard to the internal convoluted organ seen in so many of the *Actinocridæ* belonging to the respiratory instead of the digestive system, we would remark that its large size seems to us a strong objection to such a conclusion. In many instances it so nearly fills the whole internal cavity that there would appear to be entirely inadequate space left for an organ like a digestive sack, outside of it, while the volutions within would preclude the presence of an independent digestive sack there. In addition to this, the entire absence, so far as we can ascertain, of any analogous, internal respiratory organ in the whole range of the recent *Echinodermata*, including the existing Crinoids, would appear to be against the conclusion that this is such, unless we adopt the conclusion of Dujardin and Hupé, that the Palæozoic Crinoids had no internal digestive organs, and were nourished by absorption over the whole surface. We should certainly think it far more probable that this spiral organ is the digestive sack, than a part of a respiratory apparatus."

The objection here advanced does not appear to me to be a strong one. In many of the lower animals the digestive organs are of inconsiderable size in proportion to the whole bulk. In the Brachiopoda, for instance, the spiral ciliated arms fill nearly the whole of the internal cavity, the digestive sack being very small and occupying only a limited space near the hinge. These arms, although not the homologues of the convoluted plates of the Palæozoic Crinoids, have a strong resemblance to them, and are, moreover, at least to some extent, subservient to respiration. They are certainly not digestive sacks. In the recent echinoderms the intestine is usually a slender tube with one or more curves between the mouth and the anus. It fills only a small part of the cavity of the body, the remainder being occupied mostly by the chylaqueous fluid, which is constantly in motion and undergoing æration, through the agency of various organs, such as the respiratory tree and branchial cirrhi of the Holothuridea, the dorsal tubuli of the Asteridæ and the ambulacral systems of canals of the class generally. In no division of the animal kingdom do the respiratory organs occupy a larger proportion of the whole bulk than they do in the Echinodermata. The great size which the convoluted plate attains in some of the Crinoids is, therefore, rather more in favor of its being a respiratory than a digestive organ.

Professor Wyville Thomson says that inside of the cavity of the stomach of the recent Crinoid, *Antedon rosaceus*, there is a spiral series of glandular folds which he supposes to be a rudimentary liver. (Phil. Trans. R. S., 1865, p. 525). It is barely possible that the convoluted plate may represent this organ. At present I think it does not.

I believe that the reason why the convoluted plate attained

a greater proportional size in the Palæozoic Crinoids, than do the sand canals of the recent echinoderms, is that the function of the system of canals (of which they are all appendages,) was at first mostly respiratory, whereas in the greater number of the existing groups, it is more or less prehensive or locomotive, or both.

[To be continued.]

ART. VIII.—*Upon a new Spectroscope, with Contributions to the Spectral Analysis of the Stars;* by DR. J. C. F. ZÖLLNER.*

STELLAR spectrum analysis, in addition to its revelations concerning the physical constitution of the heavenly bodies, has begun most recently to claim attention in an increasing degree in another no less interesting direction. With the aid of this method the prospect is presented of proving, and under favorable circumstances also of measuring, what influence is exerted on the lines of the spectrum of a star by the components of the relative motion of the earth and star along the line uniting the two bodies.

A single consideration shows that effects which two separated bodies exert upon one another by means of a periodical impulse of a limited rapidity of propagation must be modified by a continual change of the distance separating them. It is Doppler's merit to have first, in the year 1841, recognized the necessity of this influence,† although the conclusions which he derived from it with respect to the color of the stars must be acknowledged as incorrect by reason of a disregard of the invisible parts of the spectrum.

With reference to sound this influence was proved to be conformable to the demands of theory by numerous experiments of Ballot, Mach and others.

On the contrary with reference to light it has not been possible hitherto to establish by observation a trustworthy value (sicher nachweisbare Grösser) of this influence, because even the cosmical movements, which are the greatest at our disposal for this object, are very small in comparison with the rapidity of the propagation of light.

The great improvement however, which optical instruments for the observation of spectra have experienced since the dis-

* From the Proceedings of the Royal Society of Sciences of Saxony at Leipzig, Session of Feb. 6, 1869. Translated by A. N. Skinner, assistant at the Dearborn Observatory, Chicago, Illinois.

† Doppler, "On the colored light of double stars and of some other stars of the heavens." Transactions of the Bohemian Society of Sciences, vol. ii, (1841–42) p. 465–482.

covery of spectrum analysis, presents the prospect of demonstrating this influence by the spectra of the stars. According to theory this influence must show itself in a small displacement of the lines of the spectrum. For example, for a mean velocity of the earth of four German miles per second, this displacement would amount to the tenth part of the distance separating the two sodium lines. This value which is obtained in a very simple way from the velocity of light and the undulation-time of the rays corresponding to the sodium lines, has only quite lately been derived again by J. C. Maxwell, in agreement with earlier computations by F. Eisenlohr* and others.

The amount to be observed of the displacement appeared, however, to Maxwell to be so small, that he closed his considerations concerning this (having reference to the spectroscope hitherto constructed and the method of determining the position of the lines), with the remark: "It cannot be determined by spectroscopic observations with our present instruments, and need not be considered in the discussion of our observations."†

Huggins nevertheless in his most recent memoir,‡ of which the above mentioned investigations of Maxwell are an integral part, attempted the solution of the problem in question by the use of a spectroscope with no less than five prisms, of which two are Amici's, with two flint and three crown-glass prisms.

The diminution of the light caused by so great a number of prisms permits, however, the observation of only the brightest stars. Huggins indeed even confined himself to the communication of his results from observations on Sirius, and he believed here that he found a small displacement of the line F in comparison with the bright hydrogen line produced by a Geissler's tube. The direction and amount of the displacement would indicate an increase in the distance between the earth and Sirius, and this with a velocity of 41·1 English miles per second.

If we eliminate the component of the earth's movement, which at the time of observation amounted to 12 English miles, the resulting velocity with which the sun and Sirius are moving from one another would be 29·4 English miles, or about 6·5 German miles.

Huggins himself regards this result as one affected by great probable error, an error caused partly by the great weakening of the light, already mentioned, from numerous prisms, and partly by the difficulty of comparing the coincidences of the bright lines from terrestrial sources of light with the analogous dark lines of stellar spectra. The latter have sometimes a dif-

* Heidelberg Transactions of the Phys. Med. Soc., vol. iii, p. 190.
† Phil. Trans., 1868, p. 532. ‡ Ibid., p. 535.

ferent appearance; for example, they are blurred on the edge and of different breadth, as is precisely the case with the line F in the spectrum of Sirius.

The most essential of these difficulties, which have hitherto opposed a definite solution of the problem in question, I believe that I have successfully overcome, by a new construction of the spectroscope, the first example of which I have the honor to exhibit here to the Royal Society.

The arrangement is in essentials the following. The line of light produced by a slit, or a cylindrical lens, lies in the focus of a lens which as in all spectroscopes renders parallel the rays to be dispersed. Then the rays pass through two Amici's direct-vision prism-systems of excellent quality, which I obtained from the optical establishment of Merz in Munich.

These are fastened to one another in such a manner that though each passes one half of the pencil of rays proceeding from the collimator object-glass, and also so that the refracting angles lie on opposite sides. In this way the collected pencil of rays will be dispersed in the two spectra in an opposite direction. The object-glass of the observing telescope, which unites the rays again to an image, is perpendicular to the refracting angles of the prisms placed horizontally, and as in the heliometer, is divided; each of the two halves can be moved micrometrically both parallel to the line of section and perpendicular to it. By means of this we can bring the lines of one spectrum into coincidence with those of the other, and also place the spectra in immediate juxtaposition instead of superposing them, so that one spectrum moves by the other like a vernier, or we can superpose them only partially. By means of this construction not only is the delicate principle of double-images rendered available for the determination of any change whatever in the position of the lines of the spectrum, but *any such change is also doubled*, since its influence appears in the two spectra in an opposite sense.

The principal of the reversion of spectra which lies at the foundation of the instrument described, on account of which I venture to propose for it the name "REVERSION SPECTROSCOPE," can be introduced without using Amici's systems of prisms. It is only needed to reverse one part of the pencil of rays proceeding from a common prism by reflection on a mirror or prism, and then to observe the united pencil of rays exactly as above with a telescope furnished with a divided object-glass. Furthermore, this principle renders the simultaneous introduction of artificial sources of light for the investigation of small changes of refrangibility wholly unnecessary, and permits the perception and measurement of these changes, by means of the changes in position of objects completely similar in kind.

The series of measurements which was carried out both on the dark D line of the solar spectrum, and also on the bright sodium lines of a candle flame impregnated with common salt, (and these I venture to add here to show the working power of the instrument), authorizes the hope, that with the aid of this spectroscope we shall succeed not only in perceiving the influence of the earth's movement, but also in determining it quantitatively with such accuracy as appears desirable for an approximate (vorläufig) control of theoretical conclusions.

The numbers cited denote the parts of the micrometer-screw, and refer to the distance between the two sodium lines:

Sodium flame.	Sun.
49·5	49·5
50·5	51·5
53·0	48·1
49·5	48·9
50·6±0·6	49·6±0·5

In the following series of observations the reversion spectroscope was furnished, not only with another micrometer-screw with a somewhat coarser thread, but also with two other systems of prisms whose dispersion in the region of the sodium line is 1·77 times greater than that of the systems used for the above measurements. Likewise the old achromatic object-glasses of the collimator and the observing telescope were replaced by unachromatic ones, by which not only nothing was lost in sharpness of the images, but, as was designed, an advantage was gained in clearness and distinctness by increasing the intensity of light.

Sun.

Screw divisions.	Deviation from mean.
67·1	−0·8
69·4	+1·5
68·4	+0·5
67·9	0·0
66·6	−1·3
66·1	−1·8
68·2	+0·3
68·0	+0·1
69·6	+1·7
Mean 67·9±0·3	

According to this, the interval between the two D lines was accurately determined, with a probable error of $\frac{1}{2\frac{1}{2}\frac{1}{6}}$ of its value. But in accordance with facts previously presented, a change of the distance separating the source of light and the spectroscope, with a velocity of four German miles per second, will effect a corresponding displacement of the lines of the two spectra, to the amount of $\frac{1}{5}$ of the interval of the D lines, a quantity which

is also about forty times greater than the probable error found above for the mean of nine readings.

If, therefore, in the observation of stellar spectra, a sufficient amount of light can be used, it can be definitely determined in the way stated, whether the expected displacement of the lines of the spectrum occurs or not. In reference to the intensity of light required, I venture to remark, that, for these observations, an unachromatic lens* of one Paris foot in aperture and six feet focal length is at our disposal; its cone of rays one inch from the focus is acted upon by a suitable concave meniscus of flint-glass, and, freed thus as much as possible from spherical and chromatic aberration, is directed to the slit of the spectroscope. I feel that I should here especially point out the fact, that, in the use of a slit, the achromatism of the optical image, for the observation of its spectrum, (especially of individual parts of it), is unessential, and consequently the construction indicated here must claim the important preference of great cheapness in comparison with those with achromatics of strong light. Evidently this advantage must be given up in those cases, where, as in double stars, the desideratum is the sharpest possible separation of the objects under investigation.

I may be permitted perhaps to make some remarks upon problems and methods which refer to spectrum observations on the sun and with which I am at present employed.

The sun possesses a velocity of rotation, by virtue of which a point on its equator moves with a velocity of about 0·25 German mile. If, therefore, with the aid of a heliometer, or in any other way, we produce a double image of the sun, and by a suitable arrangement bring into contact two points of the equatorial limb, then at the point of contact, parts of the sun's surface border upon one another, one of which approaches us with a velocity of the given amount, and the other recedes from us with the same velocity. From this arises a difference of velocity of the parts in contact, in the direction of the line of sight, of about half a German mile. According to previous statements such an amount of movement would cause a change of position of the sodium lines corresponding to the 80th part of the interval between them. Therefore if by combination of a sufficient number of prisms, we succeed in perceiving such a quantity, so as to measure it, we need only to bring the middle of the slit into the line joining the two centers of the images of the sun which are tangent to each other in order to see the two spectra of the sun's limbs which are thus in contact, close together in the field of view, and then, under the most favorable relations, to observe the displacement in question. In this way then the position of the solar equator, and, in case of the

* Made in the optical establishment of H. Schroeder in Hamburg.

practicability of measurements, the velocity of rotation in various heliographic latitudes would be determined, which would be of the greatest interest with reference to the opinions pronounced most recently on this point.

But even without regard to a quantitave determination of the phenomenon in question, by means of a proof of it only qualitative, even a simple means would be found of separating *all the lines which arise from absorption in the earth's atmosphere, from those which owe their origin to the solar atmosphere*, since the displacement in question could evidently extend only to the latter.

Another subject for the investigation of spectrum analysis is the protuberances. As is well known, Lockyer and Janssen were the first who succeeded, independently of a total eclipse of the sun, in observing the spectra of these forms, which consist of three bright lines.

At the present time it is the object of most earnest endeavor to discover any methods which permit not only the observation of these lines but also simultaneously of the entire shape of the protuberance.

The length of the bright lines corresponds to the magnitude of the dimension of the protuberance concerned, which occurs in the direction of the slit. Consequently if we bring the slit successively into different positions so that it cuts the protuberance in as many directions, then we are able to construct the form of the image observed, as Lockyer has already done. Thereupon Janssen has proposed the construction of a rotating spectroscope, in order thus with a sufficient rapidity of rotation to survey rapidly the form of the whole protuberance during the continuance of the impression of light.

Leaving out of consideration the mechanical difficulties of such a rotating spectroscope in which one of the bright protuberance lines must lie accurately in the axis of rotation, the design in view can be accomplished in a simpler and more complete manner by an oscillation of the slit perpendicular to its direction. In this way the protuberance could be observed in three differently colored images corresponding to the three different lines of its spectrum.

In this method, with a movable slit, the changes of brightness, through which the protuberance passes from its base, will however be considerably weakened, in proportion to the length of the path passed over by the slit; in the rotating spectroscope especially, the brightness of the protuberance itself would be weakened from the center of rotation out to the edge, and consequently the observation of the natural relations of the brightness of the image would be frustrated.

For this reason I have in view the introduction of another

very simple method for the attainment of the object in question, of the practicability of which I am already convinced by experiments on terrestrial sources of light, to be described more in detail below. The principles on which this method depends are the following:

1. The apparent brightness (glanz, *claritas visa*)* of a strip of a protuberance is independent of the aperture of the slit under the hypothesis that it continues to have a perceptible breadth on the retina.

2. The brightness of the superposed spectrum increases proportionally to the width of the slit.

3. With an oscillating or rotating slit, the brightness of the superposed spectrum remains unchanged, while that of the image of the protuberance arising from the permanence of the impressions of light, diminishes on the other hand in accordance with a law depending on the number and duration of the excitations of the point of the retina concerned, which occur in a unit of time, and also upon the refrangibility of the strip of the protuberance observed.

If, for simplicity's sake, we suppose that the entire surface, over which the slit moves in its rotation or oscillation, is filled up by the protuberance, and if we suppose also that the intensity of the image arising, is inversely proportional to that surface, (corresponding to a uniform extension, over the surface, of the light passing through the slit), then under the assumption of the above three propositions, the relation of intensity between back-ground and protuberance, would remain the same; and we may

First, reduce the brightness of the image of the protuberance by an *oscillation* of the slit, and by this leave unchanged the brightness of the superposed spectrum (by 2); or we may

Secondly, open the slit *unmoved* so far that its aperture extends at once over the space, which in the first case the oscillation extended over. In this (by 1) the apparent brightness of the protuberance remains unchanged, but that of the back-ground is increased in the same ratio that it was previously weakened. Consequently under the suppositions made, the designed object would be accomplished much more simply in the second way, if, because of its blinding, care is continually exercised that the intense light of the real body of the sun should not penetrate the slit.

The slit need be opened only just so far that the protuberance or a part of it may appear in the opening. A suitable weakening of the entire field of view must be provided for, by means of polarizing or absorbing media, which are to be placed before the eye-piece, in order that the relative intensity between the

* Lambert, Photometria, etc. §§ 36 and 37.

protuberance and the superposed spectrum may be permitted to stand out as strongly as possible to the perception.

Led by these conclusions, I have sought, with the aid of terrestrial sources of light, to realize the conditions under which the protuberances are visible, in order, in this way, to test both methods and to convince myself of their practicability. For the better comprehension of the described experiments, let the following remarks first be premised.

The reason why the protuberances are invisible under ordinary circumstances, by screening off the intense image of the sun to the edge, lies in the strongly illuminated part of our atmosphere covering the image of the protuberance. In a total eclipse of the sun this superposed light is weakened to such an extent, that then the intensely illuminated protuberances raise themselves from the illuminated parts of the corona.

We can make an approximate estimate of the amount of the reduction of the diffused light of our atmosphere required for this, if we assume the mean illumination of the atmosphere in a total eclipse equal to that of a mean full moon. According to my photometric measurements* this illumination is 618000 times weaker than that produced by the sun; consequently then the selective absorption of colored media must stand in a similar relation in reference to the homogeneous light of protuberances, if we wish to make the protuberances visible in this way without dispersion as is generally sought at the present time.

On the contrary the possibility of accomplishing this end, with the aid of the prism by the dispersion of the superposed atmospheric light, depends essentially upon the circumstance, that this light is composed of rays of all degrees of refrangibility, while that of the protuberances, however, is composed of only three homogeneous assemblages of rays.

The superposition of an unhomogeneous mass of light upon a body shining with homogeneous light and limited by sharp boundaries, I have accomplished artificially, in the following way. The wick of an alcohol flame was impregnated with chlorid of sodium and chlorid of lithium. At a distance of 18 feet before this flame, a piece of plate glass was so set, at an angle of 45° to the direction of the observation, that the reflected image of a petroleum flame at one side covered the faintly shining alcohol flame, and, by reason of its much greater intensity, rendered it completely invisible. About one foot before the reflecting plate of glass, was situated a small lens of six inches focus, which directed a small image of the alcohol flame to the slit of the spectroscope. The latter was fastened to the end of a spring ten inches long, by means of which it could be put in oscillation of sufficient magnitude for about five minutes by

* Photometrische Untersuchungen, etc., p. 105, fol., Leipsic, 1865.

first removing it from its place of equilibrium and then releasing it. Next then, the aperture of the slit was so far decreased, that, with the slit at rest, the double line D and, proportionally faint, the lithium line also appeared sharply defined in the field.

As soon as the slit was put in oscillation, these lines transformed themselves into sharp images of the alcohol flame, of which the two sodium images covered about half. The apparent brightness of these three images was much less than that of the bright lines, and in consequence of this also their relief from the diffusely illuminated spectrum ground was in the same ratio less distinct than that of the lines with the slit in a state of rest.

When now I made use of the second of the above proposed methods, and opened the unmoved slit so far that the little image of the alcohol flame was only just contained by the rectangular aperture of the slit, I was surprised by a far greater delicacy and distinctness, with which the image of the flame relieved itself from the diffusely illuminated spectrum ground. Consequently the above reduction of the apparent brilliancy of the protuberance with an oscillating slit, assumed in the theoretical discussion to be in accordance with a simple law, appeared quite strongly to incline our favor to the method last employed.

I remark here that for this experiment only one of the above mentioned new prisms was employed. But it is evident that with an additional dispersion, the diminution of the superposed unhomogeneous light can be increased at pleasure.

As may be seen, no difficulty in principle stands in the way of the application of this method to the protuberances of the sun.* Practical success, however, is dependent essentially on this, whether a sufficiently strong dispersion of light can be obtained for the actual relation of intensity between the homogeneous light of the protuberances and of the superposed light of the atmosphere. If, however, we are authorized to conclude upon a very considerable relative brilliancy of the protuberances, from the intensity and distinctness with which their lines, especially the middle one, appear,—of which I am convinced by an actual inspection on the 24th of December of the past year at the observatory at Berlin,—then the means at my command at present consisting of four excellent systems of prisms must indeed be sufficient to solve satisfactorily in the way here proposed the problem of the visibility of the protuberances.

Supplement.

According to a communication by letter, lately received from Dr. Schallen at Cologne, Mr. Lockyer also has succeeded in

* On account of an incomplete arrangement of the instrument demanded I have till now been obliged to refrain from an actual trial of this method on the sun.

observing the solar protuberances in their whole extent, according to the method developed by me in the above communication. By this communication, Mr. Lockyer has employed a spectroscope with *seven* prisms, and has communicated his results in a report held in the Royal Institution, which ought to appear in print June 15th. Since I have neither received this report up to the present time, nor even learned any details concerning Mr. Lockyer's results, it may be permitted me here to communicate the following in regard to the procedure employed by me.

The spectroscope, which has been made according to my designs in the optical establishment of Mr. Fauber in Leipsic, possesses one excellent direct vision prism by Merz. The spectroscope was fastened in a suitable way to the six feet refractor of this observatory. The height of the slit amounted to 6′ 20″ of arc, and the aperture varied with the height and magnitude of the protuberance observed. And here let it be remarked that it is most advantageous in observing to bring the length of the slit tangent to the sun's limb. By this plan on the one hand a greater segment of the sun's limb is surveyed at one view, and on the other the advantage is gained of determining with great accuracy the position angle of the protuberance, since the entrance of the sun's disc makes itself immediately noticeable by a flashing out of a narrow band-formed spectrum in the middle of the field of view. This point of the first flashing out can be easily brought to the place of the protuberance in question by revolving the spectroscope about its longitudinal axis, and in this case, as is readily perceived, the slit is tangent to the point of the sun's limb occupied by the protuberance. The actual place of the spectroscope can be read off on a divided circle, and this gives the position angle of the protuberance.

In order to bring individual points of the sun's limb conveniently before the slit of the spectroscope, two different methods can be employed. By one method the object-glass of the refractor is so fastened in a ring that its optical axis is inclined about 15′ to the longitudinal axis of the telescope. If this ring is revolved by the help of a screw on the telescope to be worked by the observer, the optical axis of the object-glass describes a cone of about 30′ opening, so that successively different parts of the limb come in focus before the middle of the slit. Of course the position of the slit must be varied in a corresponding way by a revolution of the spectroscope.

By the other method, which affords the advantage of an unchanged position of the slit, the rays before their union into an image are sent through a reversion-prism, so called. If this is rotated about the axis of the instrument, the image of the sun also revolves about its center and permits successively different points of the limb to fall on the slit. The angle of position is determined by the position of the reversion-prism.

The size of the sun's image in the refractor, or, in other words, the focal length of the object-glass employed, plays an important part in the whole method. It follows immediately from the theory of the method developed above, that *with the same spectroscope* the contrast between protuberance and back-ground is dependent only upon the width of the slit. Since, then, with a *constant* width of slit, the smaller the sun's image, so much a greater part of the protuberance is surveyed at once, it follows that we must not seek to accomplish the amplification of the protuberance by the sun's image, that is, with a long focus of the object-glass of the refractor, but as much as possible by the lens apparatus of the spectroscope. This can be easily accomplished by using a collimator with a relatively short focus to that of the object-glass of the refractor. Suppose, for example, we have a refractor of ten feet focus with a spectroscope attached, in which the focal length of both object-glasses are equally large. If now with this it is required to open the slit to the breadth of one millimeter in order to take in at one view a protuberance of a certain size, with an image of the sun $\frac{1}{10}$ smaller, this aperture could be reduced to $\frac{1}{10}$ of a millimeter, through which not only can the protuberance be seen in its whole extent, but also with a ten times greater contrast with the spectrum back-ground. In order now to obtain again the amplification of the protuberance *in the field of view*, which was lost by the diminution of the sun's image, the focus of the collimator need only be made ten times shorter than that of the spectroscope. To continue with the example proposed, a ten times better effect would be obtained, *giving the same optical amplification* of the protuberance *with the same prism systems*, if there is chosen in place of the ten feet refractor a telescope of only one foot focus, and for the focal distance of the collimator about two inches, and that of the observing telescope about twenty inches. The quality of the images in this is influenced very little, as far as affected by the lens system, since the defect of chromatic aberration does not enter at all on account of the homogeneity of the light of the protuberances; consequently, properly selected unachromatic lenses can be used for such combinations without any hesitation, as I have convinced myself by numerous trials. The extraordinary compactness which such instruments for the observation of the solar protuberances possess, permits a very delicate clock movement, and presents the prospect of realizing in this simple way the idea expressed already in my former communication, namely, an artificial total eclipse of the sun, of an arbitrary length, for the observation in the future of all the protuberances present on the sun's limb at the same time.

Leipsic, August 26, 1869.

ART. IX.—*Polarity and Polycephalism, an essay on Individuality;* by H. JAMES CLARK, A.B., B.S., Prof. Nat. Hist. Kentucky University; Lexington, Ky.*

WE have already, in a general work† upon the development, morphology and classification of animals, entered our protest against that theory of individuality which assumes that the medusoid genitalia of Hydromedusæ should be considered as individuals in a higher sense than the hydræ are, no matter to how low a degree of development they descend nor at how high an elevation they arrive in the complicity and differentiation of their parts. We still adhere to that protest as far as the hydræ and medusoids are related to each other; but look upon them both in a modified light in reference to their individuality. We suppose it will not be questioned that, in the main, naturalists and physiologists have always defined in their own minds, and in their teachings, the zoölogical *individual* to be a *monocephalic* being; that they have taken as their standard the most highly developed creatures of the animal kingdom, whose *oneness* and independence place them on an equal footing with man in these respects. In the discussion of late years upon the individuality of the lower, compound, colonial denizens of the water, the main points at issue have always been to determine whether a certain form was, on one hand, an *individual*, either in its highest sense (a monomeric, independent integral) or one of several interdependent individuals which constitute a colony (a polymeric integer), or, on the other hand, was an *organ*, which formed only a part of an individual, whether the latter be monomerous (as in Hybocodon and Corymorpha producing free medusæ), or polymerous (as in Coryne with free medusæ).

The possibility of a third category of individuality had not arisen in the minds of philosophic naturalists until the question of the bilaterality of the two lower grand divisions of the animal kingdom had been discussed so vigorously, and elevated to such a prominence among the theories of the day as to extend its influence even to the determination of the oneness or duality of the members of the highest of all grand divisions, and indeed the highest of all animals, man himself. Here at this point we find breaking in upon us the Teratological essays of St. Hilaire, and the more recent decisions of Wyman upon the same subject, with the strange confirmations of Lereboullet, by his discoveries of the fissigemmation of the piscine egg, and

* This is an extract from a forthcoming memoir on the anatomy and physiology of Lucernariæ.

† Mind in Nature, or the Origin of Life and the Mode of Development of Animals. New York, D. Appleton & Co. 1865.

the evolution of two heads or two tails from one center of development,—the *dualistic* tendency of the highest vertebrate emphasized by the presentation of the living, tangible reality.*

Such possibilities among the vertebrata staring us in the face could not but send the thoughts flashing back among the inferior, less determined, less differentiated organizations; and the mind's eye needed not to dwell long among the many-headed Polypi and Hydromedusæ, the Crinoidæ, Bryozoa, Ascidiadæ, Pyrosomidæ, Salpæ, &c., before discovering a multitude of more than shadowy tendencies; it became fixed upon numerous sharply and clearly established, unmistakable dualities and pluralities; all arising from one common center, the *ovum*. Had we not the problem of plural individuality solved here,—*a polycephalism—?* the diffuse vitality of the animal-egg of the lowest ranks of life outspoken in the indetermined number and localization of the subdivisions of the Polyp or Hydromedusa corporation; and even the organization itself undecided as to whether it should exemplify its oneness in a simple unit of form, as in the *pseudoindividuum* of Bryozoa, Ascidiæ, or resolve its offices and configuration into the repetitive, multiplied sameness of the sexless and sexual *proles* of Salpæ, Tæniæ, Annelidæ and Hydromedusæ, or the excessive reiterations of the genitalia of Polypi.

The old type of monomerism, the vertebrate individual *par excellence*, has then become the modern, more than transcendental *duality*. The originals of multitudes of figures in St. Hilaire's 'Teratologie,' of the memoir of Lereboullet, and of the condensed aphoristic sketches of Wyman stand forth the real, material embodiments of the idea upon which all sentient life is founded. *Bilaterality* does not express the thought, it embraces too little; it is to be classed with antero-posteriority and dorso-ventrality, to signify the *subdominant* features of the animal architecture; features which evolve themselves as the concomitant resultants of the development of the primitive dominant which originally gave shape to *bipolar ovum*. The embryologist, and to his thoughts the subject is most germane, reflecting upon the physical aspects of the forming egg, would naturally arrange its features in two antagonistic fields; and thereupon attempting to define their position in regard to the contour of the concrete sphere, almost inevitably would give utterance to the word *polarity*. This is the dominant, main idea of sentient life.

It is *polarity* which is evinced when the self-dispersing, self-repellant potentiality of the animal-egg lays down the right and left of the germ on opposing sides of a line; when the cephalic and caudal areas grow in opposite directions from a common point of emanation; or when the animal and vegetative

* See remarks of the author on this subject in "Mind in Nature," ut Sup., p. 85.

foundations project themselves into diametrically diverse, dorsal and ventral spaces. Each and all of these phenomena have a common point to rest upon; and they proclaim, by their mode of operation, the controlling influence of a power which, fixing itself upon that point, as it were, radiates itself through the whole organism, and disposes its several features in such a way that they all display, either in mode of evolution or by a direct connection, a polar tendency; a growing out of one pole and a dispersion toward the opposite one; features most developed and decided in configuration next the point of departure, and least developed and most diffuse and indeterminate in the opposite area; the latter always through life standing in the same relation to the former as supply does to demand, as nutrition does to the power which regulates the absorption of the nutriment.

But *bilaterality* carries with it something more than the sheer dextral and sinistral opposition of the lateral halves of the body; it is not merely the bipartition of a *unit* of form; for the distal as well as the proximal edges of these halves,—the free borders and the margins of contact—are mutually interchangeable; the former may take the place of the latter, and yet leave the apparent bipartite unit undisturbed in internal relations.

Antero-posteriority exhibits the same interchangeability as bilaterality, but, although plainly enough, not so conspicuously in a comparative, homological sense as in the physiological interplay of the functions, such as we see in the relations of the allantois to respiration in the embryo, or in the ratio of excretion of the renal organs dependent upon the degree of activity of the respiratory and perspiratory functions; or in the relation of the reproductive organs to the vocal and respiratory, when the former are in an abnormal condition, or when they change from one period of life to another, from youth to adolescence; and in many other interdependent relations familiar to the morphologist of the present day.

Bilaterality, antero-posteriority, and dorso-ventrality, the three principal *subdominants of polarity*, have a very methodical disposition, and are quite pronounced and sharply defined among the higher groups of animals,—the more seemingly *units* of organization,—but if we go to the opposite extreme of grade we shall find, among the lower classes of life, that the polaric element (like the differentiation of organization, and that of function) is in an almost elementary condition, expressing itself vaguely in the scattered heads of a branch of Coryne, or Tubularia, or Clavellina; or a little more determinately in the distichous arrangement of the hydra heads of Dynamena and Sertularia, or in the singularly stellate disposition of the zoöids of Botryllus, with their common cloacal orifice.

When, however, polymerism, in its usually accepted sense, fails, as it does step by step in the gradually rising degrees of rank, polarity gains the ascendency in point of regularity and the closer intimacy and symmetrical arrangement of the components of the organization which it holds sway over. Thus it is that two, or more, scattered, consimilar parts, or complete organizations may combine to form a seeming *one*, an apparent, bipartite or multipartite *unit*. The multiple repetition of heads among the lower polymeric kinds is here reduced to a dual repetition, and the parts condensed into one form an approximative unit, a zoölogical *individuum*, as the highest expression of unity attainable by the vertebrate *zoön*.

The duality, nay the plurality of the subdivision of the vertebrate axis, as illustrated by the embryo fishes of Lereboullet, is recalled in the diffuseness of the many hydræ of the dendritic Campanulariæ, or disguises itself under the interminable heteromorphism of the Siphonophoræ; it is polymerous but dimorphous in Salpa, or polymerous but monomorphous in the fresh water Polyzoa; temporarily a polymerous, monomorphic *individuum* in the fissigemmating Hydra, it eventually resolves itself into disconnected *pseudo-individua;* for a time polymerous but dimorphic in the annelidan Myrianida of Milne-Edwards it finally assumes the appearance of a true, self-contained *individuum* in each one of the separate, independently moving *sexual segments*, and in the original budding-stock (the direct legitimate offspring of the egg) from which they shot forth.

The thorough historian of the multifarious, so-called alternate, generations of the Acalephæ will see nothing but a generative organ in the spermatic and ovarian sacs of Hydra; and detect nothing more in the grape-like clusters about the base of the head of Clava, or in the grouped moniliform projections behind the corona of tentacles of Eudendrium. The polymerism of these organs of Eudendrium is merely, and nothing more than a repetition of the simple sac of Clava; the diversity in form is only apparent. But one step higher in complicity and our observer will find in the tentaculiferous terminations of the reproductive sacs of Thamnocnidia and Parypha a premonition of a forthcoming cephalic independence, such as is already fully exemplified in the many hydras of the polymeric, dendritic mass. A similar progression toward cephalic freedom will also be seen in the simplest generative sacs of Laomedea amphora, and L. flexuosa, &c., and rising through successive degrees of complicity to those of Gonothyrea (Laomedea) Lovenii *All*, which not only are tipped with tentaculate processes as in Parypha and Thamnocnidia, but have developed within them a series of longitudinal tubes, like those in the homologous organ of Tubularia indivisa.

Gradually and methodically the progressive steps of complication lead on, with a more and more marked separation of the genitalia from a direct relation to the general mass, or even to the hydræ in particular, whilst a consentaneous development gathers around them and brings them into immediate alliance with an envelope whose *morph* is only a slightly varied repetition of that of the hydra, but whose greater degree of complicity gives it a better claim to be ranked as the highest among the *cephalic subdivisions* of the body. But the full aim of the train of development is not divulged here; its results only exemplify a part of it in the predominance of the reproductive function and a differentiation of the nutritive cavity into distinct channels of circulation, and the subordination of a definite region of its periphery to a tentacular, prehensile office.

Step by step, however, all the elements of a complete organism are successively absorbed out of the primitive hydra-mass, and remodelled into the fashion of a medusoid; the reproductive character has become a less obtrusive feature; motion attracts attention above all others; prehension has full sway in the highly developed tentacles; and the latter point, like fingers, to the self-sustaining power of the acalephan morph in the complete organization of the longitudinal and circular chymiferous channels, opening into the receptive cavity of a highly flexible, proboscidal *manubrium*. The preliminary processes of fissigemmation are complete; the primary genesis of the *ovum, in its integrity*, is finished; the primitive stock has become differentiated into two widely diverse varieties of one morph, the *hydroid cephalism* and the *medusoid cephalism*. Such is the condition at which the hydromedusariæ of Corymorpha, Hybocodon, Ectopleura (Tubularia) Dumortierii, Pennaria, Coryne mirabilis, Margelis, Bougainvillea, and many Campanulariæ have arrived previous to the disintegration of their mass into the free *pseudo-individual medusoids*, and their less independent, contemporary homologues, the persistent *hydroid cephalisms*.

No one holding the present prevailing views in regard to individuality would find a difficulty in seeing that the members of a chain of Salpæ are so-called *individuals*, notwithstanding they are attached obliquely end to end, and *organically* connected. Now although the self-dividing worm, Myrianida for example, the so-called *asexual* stock may become, by actual separation, two individuals, apparently, viz: *sexless* and *sexual*, yet once they were more closely connected organically than the Salpæ which do not separate. Is now the closer connection of the yet unseparated asexual and sexual parts of the worm to make them less distinct individuals than those of the Salpa? It would seem so, according to the advocates of individualism;

and therefore the Myrianida, with its posterior string of six or seven consecutive sexual *buds* is a *monocephalic* individual. But in the sexless Salpa-form budding the sexual chain we have a closer parallelism with the worm than in the chain alone, in fact an identity of relation; and yet for all that we would not think of calling the stock (sexless) and the chain (sexual) together one individual, with *one* head, but rather *many* headed, or in other words a *polymeric unit or individual*, of sexual and sexless *cephalisms*. Therefore by a parity of reasoning we ought to denominate the Myrianida and its buds as a succession or series of *cephalisms*. The fact that the worm components are more in one line than in the Salpa only makes an *apparently* more individualistic body. Among tapeworms the several heads (*cephaloids*) of the *scolex* (Cœnurus) of Tænia Cœnurus are not arranged in a line end to end, but all are free anteriorly, and connected with each other posteriorly by a common body. The closer connection of the subdivisions of the annelid is only one of degree; and as to having more organs in common than the Salpa it is rather like the community of interest which the coral cephalisms have in the main trunk.

Since the sexual and sexless are necessary to make up a complete organism, i. e., vegetative and reproductive, the one a completement of the other, neither *alone* can represent the *individual* unit, or whole cycle of life: and CEPHALISM therefore is a better term to indicate the potentiality of these subdivisions to live apart, although it does not always occur (as in Corals, Bryozoa, some Campanulariæ and Tubulariæ), or when apart (as in other Tubulariæ [T. Dumortierii], Laomedeæ [Eucope diaphana, &c.,] and Salpæ, Myrianida, &c.,) meaning more or less *incomplete* individuals (pseudo-individuals) which are either mainly vegetative or mainly reproductive, as the case may be.

We look upon cephalism then, on one hand, as having a controlling influence of a low degree of independence when shared in common by the multiple heads of a coral polypidom, and, on the other hand, as attaining to the highest independence as a controlling power, when the multiple parts of a so-called compound individual separate from each other, and are singly under the influence of this power. The latter obtains when Hydra or Actinia separates its buds from itself; or when the sexual part of the annelid worm subdivides from the asexual one. Cephalism of a low degree is more readily recognized in the aggregated cephaloids of Salpa than in the undivided worm, but, unlike the latter, remain connected cephaloids (in the chain) when separated from the budding stock.

By thus dividing the body of a Hydromedusa into two parts, which shall contain, severally, the vegetative dominant (*i. e.*,

vegetative cephaloid) and the sexual dominant, (*i. e., sexual cephaloid*) we avoid the absurdity of assigning individuality to the *egg-sac* of Hydra and others of its allies which have evidently a mere genital organ. Although we might be inclined to admit that some *cephalisms* may gradually become complete individuals, as when the buds of Actinia or Hydra separate from the parent body; on the other hand we must insist that an *individual* cannot retain the same significance when reduced to a mere *genital organ*, as when, in Coryne, a free medusoid (Sarsia) later in the season becomes an egg-sac; or the free medusa of Tubularia (Ectopleura) Dumortierii is represented in Tubularia (Thamnocnidia) spectabilis and Parypha crocea by a plain sac; or in Siphonophoræ where a subdivision may be either a sexual medusoid, or a sexless swimming-bell, or a mere "scale."

Under the term *cephalism* we include two forms, or *morphs*, viz: (1) the *cephalid*, or such subdivisions of a body as have a complete organization, whether united in common (as in some Vorticellidæ, Corals, Bryozoa, Crinoidæ, some Ascidiadæ, and Pyrosomidæ) or separating singly from the main stock (as in Hydra and Actiniæ), and (2) the *cephaloid*, or those divisions of a fissigemmating body which do not contain a complete organization, and may be either mostly *sexual* (as the so-called medusa of Hydromedusæ, or the posterior divisions of Myrianida and other worms, or the joints of Tænia, or the Cercariæ brood of Distoma, or the chain of Salpa) or mostly vegetative and *sexless* (as the hydra of Hydromedusæ, the Myrianida stock, the head of Tænia, the single, budding stock of Salpa or the budding Cercaria-nurses of Distoma.)

ART. X.—*On Laurentian Rocks in Eastern Massachusetts;* by Dr. T. STERRY HUNT, F.R.S.

IN a paper read before the American Association for the Advancement of Science at Washington in April, 1854, and published in this Journal for September in the same year, (vol. xvii, page 193,) I noticed the crystalline limestones of northeastern Massachusetts, which were described by the late Dr. Hitchcock as enclosed in the great gneissic and hornblendic formation stretching through that portion of the state. These limestones, which are met with at various points from Bolton by Chelmsford on to Newburyport, present a close mineralogical resemblance to those of the Adirondacks and Laurentides, and also to those of the Highlands of New York and New Jersey, a resemblance which extends to the gneissic rocks which in

these various regions accompany the crystalline limestones. I, at that time, accepted without examination the view maintained by Mather and H. D. Rogers, that these limestones in southern New York and New Jersey were altered Silurian strata, although mineralogically identical with those farther north of undoubted Laurentian age. Led by this conclusion to attach comparatively little importance to mineralogical and lithological resemblances, and guided by other considerations given in the paper just referred to, I then suggested that the crystalline limestones and their accompanying rocks in northeastern Massachusetts might probably be of Devonian age. The subsequent investigations of Hall, Logan and Cooke in the Highlands of New York and New Jersey have however left no doubt that these supposed altered Silurian rocks are really of Laurentian age, and led me to suspect that the same might be the case with those of eastern Massachusetts. This view, which was shared by Prof. James Hall, I ventured to put forward at the meeting of the American Association for the Advancement of Science at Salem in August, 1869, when I showed that it was probable, not only on lithological grounds, but from the fact that the Laurentian rocks appear to the southward of the great paleozoic basin in New Brunswick and Newfoundland, which are geologically but a northeastern prolongation of New England, and moreover from the outcropping of the lowest Silurian strata at Braintree near Boston. A few days later I visited Newburyport, and in company with Dr. Henry C. Perkins of that place, had, for the first time, an opportunity of observing the gneisses and limestones in question. Their aspect confirmed my suspicion of their Laurentian age, and led me to suggest to him the propriety of searching for *Eozoon Canadense* in the limestones which there occurs mingled with serpentine. Specimens of it were thereupon placed in the hands of Mr. Bicknell of Salem, well known as a skilled microscopist, and shortly after it was announced by Dr. Perkins that Mr. Bicknell had discovered in them the Eozoon. This notice, which appeared in September in a Newburyport journal, is reproduced in the American Naturalist for November. My own specimens collected in August last near Newburyport, at the locality known as the Devil's Den, did not however furnish any traces of Eozoon, and I may here remark that I had already, so long ago as 1864, caused slices to be made of a specimen of limestone from that locality, which were then examined by Dr. Dawson with negative results. In November, however, Mr. Bicknell visited Newburyport and got from a quarry about a quarter of a mile distant from the place just mentioned, specimens of a serpentinic limestone in which he again found Eozoon. Slices

which he has kindly sent me have also been examined by Dr. Dawson, who confirms Mr. Bicknell's observation, and finds in them *Eozoon Canadense*, though fragmentary and not very well preserved. The tubuli, as in the specimens from Grenville, are injected with serpentine, and may be seen on etched surfaces as well as in transparent slices. A crystalline mineral is however abundantly disseminated in the limestone, and unskilled observers might have difficulty in recognizing the fossil.

Another locality, about twenty-eight miles to the southwestward of Newburyport, has however afforded me much better specimens. In company with Mr. L. S. Burbank of Lowell, a zealous and successful teacher of geology and mineralogy, I visited in October last, the limestone quarries of Chelmsford, some five miles from Lowell. This limestone and its accompanying gneiss closely resemble the Laurentian rocks of other regions, and scapolite, apatite and serpentine occur as associated minerals, though the latter was rare in the quarries then visited. A few days afterward Mr. Burbank kindly sent me specimens of a mixture of limestone and yellowish-green serpentine from another quarry in the vicinity, which I had been unable to visit, and these have proved to be rich in *Eozoon Canadense*. The continuous and complete calcareous skeleton of the fossil does not appear in these specimens, which seem like some portions of the rock from Grenville, as described by Sir W. E. Logan, to be made up of fragments of the calcareous shell of Eozoon, mingled with grains of serpentine, and cemented by crystalline carbonate of lime. In the specimens from Grenville, and from most other localities, the mineral matter replacing the sarcode and filling up the canals and tubuli in the calcareous Eozoon skeleton, is generally serpentine or some other silicate. Both Dawson and Carpenter however, it will be recollected, found that in the fragmentary Eozoon from Madoc, and in some small portions from Grenville, the injected mineral was like the shell itself, pure carbonate of lime, though readily distinguishable by differences in texture and transparency from the shell. Such is also the case with all the Chelmsford specimens yet examined, which abound in fragments of shell exhibiting in a very beautiful manner the cylindrical diverging and branching tubuli. The accompanying serpentine is disseminated in grains, but has no connection with the organic forms, so that, unlike the specimens in which it is the injecting mineral, the structure of these cannot be brought out by etching with acids.

These specimens from Chelmsford, it should be said, have been examined and satisfactorily identified by Dr. Dawson. The argument from mineralogical and lithological resemblances in favor of the Laurentian age of the limestone in question is therefore now supported by the undoubted presence in them of

Eozoon Canadense. In this connection it should be said that the crystalline rocks of Newburyport and Salisbury, though separated in Hitchcock's geological map from the gneisses to the southwest, and united to the syenites of Gloucester and Rockport, seem to me very unlike the latter, and closely related lithologically to the gneiss of Chelmsford, which encloses the crystalline limestone. The crystalline limestones occurring with gneissic rocks near Providence, Rhode Island, merit a careful examination for Eozoon, inasmuch as from their lithological characters they may with probability be supposed to be of Laurentian age.

Montreal, Dec. 13, 1869.

ART. XI.—*Contributions to the Chemistry of Common Salt: with particular reference to our home resources;** by C. A. GOESSMANN, Ph.D., Professor of Chemistry, Massachusetts Agricultural College, Amherst.

HOWEVER chemists and geologists may differ in regard to the methods by which chlorid of sodium has accumulated in the course of time within the waters of the ocean, there is at present but little dissent from the opinion, that the ocean has at all times been charged with salt, and that the saline residues of the oceanic waters of former geological periods, together with those of the present day, furnish us with our natural sources of supply.

The salt of commerce is chlorid of sodium more or less contaminated with various saline admixtures. These foreign substances may differ in *quality* or *quantity*. The differences in the kind of the foreign admixtures are due to the peculiarities of the source used for the manufacture. The differences in the *quantity* of the impurities, so far as the same kind of saline compounds is concerned, are determined not only by the condition of the source, but also by the mode of manufacture and by the amount of care bestowed upon the working. The fitness of a salt for domestic and industrial purposes depends quite frequently not less on its mechanical condition, than on its chemical purity; and as the composition of its natural as well as its artificial solutions exerts a most decided influence on both, it seems but proper that I should briefly consider the chief foreign saline compounds usually associated with the chlorid of sodium. To do this, we must go back to the primitive condition of our planet.

Accepting the theory that our earth has gradually passed

* Read before the National Academy of Science at its Northampton meeting, August, 1869.

from a gaseous to a solid condition, we may assume that the formation of chlorid of sodium took place mainly during the last stages of its consolidation, at a period when the more volatile elements reacted upon each other, in consequence of the diminution of temperature; that subsequently water caused the oxydation of numerous chlorids and sulphids, the acids of sulphur and of chlorine which thence resulted acting upon the solidified surface of the new planet; and, that finally these more or less violent physical and chemical revolutions resulted in the formation of the primitive ocean.

A more prolific field for speculation concerning the kind, the extent, and the order of succession of the chemical reactions, which may have preceded or accompanied the formation of this first ocean, cannot readily be conceived, since more complicated relations of matter and force can scarcely be presented for investigation. Speculations upon the last stages of the chaos, therefore, are very likely to lead us into a chaos of chemistry; for—to point out but one circumstance—the very important question as to the relative intensity of the various chemical agencies which may have been in action at any given point, is, and will always be, a mere matter of conjecture. Inasmuch as all our knowledge concerning chemical affinity, has been obtained by experimenting, under well defined conditions and on a comparatively very limited scale, it is evident that the application even of well established chemical laws to the determination of the violent reactions of the atmospheric agencies of that period upon the solid crust of our earth, should be made with caution. We are not without ingenious statements concerning the chemistry of that stage of our planet's history; yet, as their details in many instances are and always will be subjects of controversy, I deem it advisable to give merely an outline of those probable chemical and physical changes which bear directly upon the question here under discussion.

Taking into consideration the peculiar constitution of primitive rocks, their present surface condition, and the predominance in them of silicic acid, in connection with the probable character of the primitive atmosphere, there appears to be some force in the suggestion, that the decomposition of the silicates of iron, of alumina, of lime, of magnesia, of potassa, and of soda, by means of sulphuric acid, and particularly of hydrochloric acid, may have furnished the saline constituents of this primitive ocean. The compounds resulting from this chemical action being soluble in water, at least in part, were continually carried away in solution and accumulated finally in lakes and oceans. The chemical composition of those oceanic waters was dependent, therefore, on the nature and the extent of the surface disintegration, the concentration as well as the relative proportion of

their saline constituents being very different from that found at present. But on the other hand we must concede, that an entire elimination of one or more of these constituents could only be accomplished by such an alteration in the physical and chemical condition upon our planet, or in the nature of the disintegrating agencies, as would create new affinities of sufficient power, to alter the originally existing compounds. The composition of the present ocean, as compared with that of saline deposits of a more ancient date, requires the assumption of such revolutionizing causes; causes, however, which may be looked upon as merely a natural consequence of the lapse of time during the history of our earth. The same agencies in fact, which are still at work in effecting changes in the character of the saline compounds of the present ocean, suffice to explain the gradual transformation of the primitive ocean into that of the present day. The mineral acids, the presence of which we had reason to suspect in the primitive ocean, became neutralized by degrees, and ceased to react upon the newly exposed rock; and as the temperature diminished, a new disintegrating agent, carbonic acid, became active. This acid, then so abundant in the atmosphere, aided by water and oxygen, and later by vegetable and animal life, though a slower is by no means a less powerful agent in effecting the decomposition of exposed rocks; it has enriched and is still enriching the ocean with saline compounds. Carbonates and silicates of alkalies and alkaline earths were in this way introduced into the oceanic waters; and thus a gradual removal of metallic and earthy oxyds, and in some cases of those of the alkaline earths also, was effected. The sulphuric acid, exchanging the oxyds of the earths and the metals for lime, formed a less soluble sulphate, so that the amount present became dependent upon concentration and temperature. The chlorine changed from its combinations with earths and metals, only to the equally soluble compounds of the alkalies and alkaline earths. Taking the chlorid of calcium, for example, which thus far has been noticed in every salt deposit of antetertiary date, and which must have been therefore one of the original constituents of the ocean from which the salt originated, we find it necessary to believe that the chlorids must have always exceeded the sulphates; since an excess of any sulphate, except sulphate of lime, would have caused the decomposition of the chlorid of calcium.* An increase of the chlorids of the alkaline earths, as of calcium and magnesium, and of the alkalies, particularly of sodium, if we may judge from the present composition of the ocean, was the final result of the

* The supposition that chlorid of calcium was one of the primitive constituents, can only lose its force when some other general cause for its universal diffusion can be pointed out.

changes now indicated. The agencies which produced these changes are still at work; and hence the oceanic waters of the present day are liable to similar alteration in composition. There are reasons to suppose that for some time past one of the main changes has been the decomposition of the sulphate of lime by carbonate of magnesia, forming carbonate of lime, sulphate of soda and chlorid of magnesium. The latter compounds are characteristic of the salt deposits of recent date, as well as of the present marine waters; they could make their appearance only after the chlorid of calcium had been removed. There are of course various other means, by which this result may have been accomplished; but I prefer to confine myself to the one now given, as being of particular interest in view of the fact, that chlorid of calcium is a characteristic constituent of the ante-tertiary ocean. These changes in the oceanic waters extend apparently over long periods of time; they are more likely the result of a prolonged than of an intense action,—even from the beginning. For these reasons it must always be quite difficult to point out with anything like a certainty, the condition, and the composition of the ocean waters, during the successive stages in the geological development of our planet. All we can venture to assert upon that point may be summed up in the following statements:—*first*: the similarity of certain extensively distributed metalliferous, and other deposits within the crevices and fissures of primitive rocks, finds an apparently satisfactory explanation in the assumption, that their formation is due to a more or less localized precipitation, under circumstances similar to those we have supposed to exist in the primitive ocean; *second;* the saline compounds contained in the oceanic waters of ante-tertiary date appear to differ essentially from those of the present day; the solutions of the primitive saline deposits of distinctly ante-tertiary date containing in every instance, more or less chlorid of calcium, while the waters of our present ocean are characterized, instead, by an excess of soluble sulphates, as sulphate of magnesia and of soda for instance, which renders the existence there of chlorid of calcium impossible. We do not hesitate on the strength of these observations, to speak in general terms of a primitive ocean,—of an *ante-tertiary* or *Silurian ocean*, and of a *Post-tertiary ocean*, including in the latter that of the present day.

Whenever during the various geological epochs a larger or a smaller body of salt water was cut off from the main ocean, either in consequence of a receding of the ocean, an infiltration into natural basins, or of changes in the level of the strata, and was subsequently placed under favorable climatic conditions for its evaporation and the subsequent preservation either in whole or in part of its saline residue, then a salt deposit was pro-

duced. Such saline residues, commonly known as *rock salt*, have been found in almost every geological horizon, from the Silurian upward, and in many localities they are still forming at the present time. Sometimes several independent deposits occur one above the other, interstratified with the rocks of the same geological basin.

Most of the foreign salts which accompanied the chlorid of sodium in the marine waters, being more soluble than the chlorid of sodium itself, accumulated during the evaporation in the residual liquid, commonly known as mother-liquor, which covered, at least in part, the separated crystalline mass of chlorid of sodium and sulphate of lime. The amount of these salts and the manner in which they overlie the solid saline mass, is dependent on the form and the shape of the basin in which the deposits accumulated. As these mother-liquors uniformly contained a large number of deliquescent compounds, we must assume very favorable climatic and meteorological conditions to account for their evaporation, since the rate at which this took place, must have exceeded by far that of the precipitation of moisture. Whether such a state of the atmosphere was the general rule in former ages, is of course quite uncertain. Moreover, although actual investigations have demonstrated the entire absence not unfrequently of the upper layers of such saline deposits, this fact cannot be admitted as an argument against the assumption of a common marine origin of all salt deposits. For, even granting that in former geological ages, the extraordinary state of the atmosphere did exist, which would be required to effect the solidification of the entire saline mass—mother-liquor and all—there remain numerous subsequent influences, by which a part or the whole of the saline residue of the original mother-liquors, even after their complete solidification, could have been removed from a saline deposit. In fact, considering the many casualties to which these saline accumulations are liable in the course of time, it would be strange indeed, if many entire and well preserved marine salt deposits should be found.

We may assume then with some propriety, in cases where salt deposits are found without their associated foreign saline compounds, that these have been lost, either by oozing out, never having been solidified, or after solidification, have been removed by the percolation of surface waters, by subterranean currents, by peculiar secondary chemical and physical reactions or by erosive action on the surface layers. In all probability quite as many deposits have been modified in their physical and chemical characters by the subsequent elevation of their enclosing strata, a circumstance which must have favored the percolation of surface moisture, as have been changed by

denudation.* Indeed these changes have sometimes gone so far as to leave nothing but the layers of gypsum which are the substratum of all well investigated rock-salt deposits.

As the circumstances necessary for the preservation of entire salt deposits, or saline marine residues can scarcely have been the same, even throughout the same geological age, it is not surprising, that these deposits so frequently differ in their physical and chemical characters. In fact there is at present but one salt deposit known, and its discovery is of quite recent date (Stassfurth), where the entire saline mass of the ocean has been preserved; in this deposit the various saline compounds are to a large extent arranged in layers, which correspond closely with the degree of their solubility; yet even in this one instance there are facts noticeable, which leave no doubt that portions of the deposit have been exposed to peculiar disturbing influences at a later date.

But not only may salt deposits be placed under conditions which permit their re-solution, there may be in addition a re-evaporation of this solution, producing a secondary salt deposit. This requires only a repetition of the circumstances, which favored the formation and preservation of the original one. A primary salt deposit may be dissolved entirely or only in part; in either case the solution may or may not be changed in composition by filtering through the adjacent beds. Both primary and secondary salt deposits may occur in the same geological (*Post-Silurian*) formation. Secondary reactions of a physical as well as a chemical nature too may alter the character of the surrounding geological formations, and thus may indirectly affect the composition of a salt deposit, as is the case for instance at Stassfurth. The conditions which have now been given may suffice to explain to us the great variations we notice in the chemical composition both of rock salt and of brines; they may also serve as a suitable illustration of the great risk we incur, when we assume to draw conclusions of an absolute character from the geological formation in which the salt deposit has been found, as to the chemical composition of the oceanic waters of that geological age.

With these preliminary remarks upon the origin of chlorid of sodium and its associated salts, I pass on to consider the main sources of supply of common salt, with particular regard to those of this country.

The leading sources of supply for the manufacture of salt, as already stated, are three in number, *Rock salt*, *Brines*, and *Sea-water*. A. *Rock salt:* From what has been said it is manifest that rock salt from different localities may differ widely, both in

* As the rock salt deposit of Petit Anse, Louisiana, for example, and most likely that which furnishes the brines of Onondaga.

physical and chemical properties. Usually it occurs either in densely aggregated masses of distinctly cubical crystals, or in compact masses having a conchoidal fracture; it is frequently colorless and transparent, yet is more often either red, yellow, or blue, rarely green. Its most frequent saline admixtures are 1st, sulphate of lime, the chlorids of calcium, magnesium, and potassium, and the bromid and iodid of magnesium; or 2d, the sulphates of lime, magnesia, and soda, the chlorid, bromid, and iodid of magnesium, etc. A rock salt, which contains more than from 2 to 3 per cent of these impurities is unfit for domestic uses; and a salt which contains carbonate, nitrate or borate of soda, or similar foreign substances is, especially in its natural state, frequently unfit even for many manufacturing purposes also.

Rock salt deposits are frequently intercalated with layers of gypsum and clay, a fact due to the successive periods of evaporation; such conditions require especial care in mining the salt. Colorless and dry rock salt deposits having a fair composition and easy of access are mined directly with advantage; the salt obtained thereby, after being brought into a desirable form, is directly applicable to most domestic purposes, particularly meat packing. Colored salt deposits, or those which suffer from percolating waters, or which contain a large amount of foreign saline admixture, or clay; or finally, those which are located at very great depths, if worked at all, are usually dissolved, and their solutions treated like brines.

The northern part of this continent contains numerous salt deposits; some quite recently discovered, like that upon Petit Anse Island, Vermilion Bay, Louisiana, the one in Canada West, at Goderich on the shores of Lake Huron, and also that of Neyba, St. Domingo, West Indies, being of particular interest. Newspaper reports too tell of mountains of rock salt in Arizona and Nevada. The salt deposit at Goderich is buried in the shales of the upper Silurian (T. S. Hunt), at a depth of from eight to nine hundred feet; it is about forty feet thick, covers so far as present indications show, dozens of square miles, and is in close proximity to Lake Huron; its solution furnishes the superior brines at Goderich.* The salt deposit of Petit Anse, Louisiana, is apparently imbedded in Quaternary formations (E. W. Hilgard), and is covered merely by a diluvial drift from 16 to 18 feet in thickness; its extent is unknown, having been but partially explored. It is accessible by sea and by land, and is within 275 miles of the mouth of the Mississippi river. This Petit Anse rock salt, so far as at present exposed, is one of the

* Report on the Salt Resources of Goderich, Canada West, by Chas. A. Goessmann; Syracuse, N. Y., January, 1868.

purest on record.* At the present time there is but little rock salt, as such, used in the United States. Natural solutions of rock salt furnish us with the brines of Saltville in North Western Virginia, of Goderich, Canada West, and, as I believe, of Onondaga, N. Y.

B. *Brines.*—Brines are either natural or artificial; that is, they are either natural solutions of saline deposits, or they are made artificially by dissolving rock salt. In regard to natural brines we are quite frequently ignorant of the exact location, the extent and the nature of the saline mass from which they originate, while in the case of artificial brines, we are familiar at least to some extent, with the nature of the sources from which our supply is drawn.

Brines differ in strength and in composition, scarcely two of them being alike. In strength they vary from three to twenty-six per cent of saline matter, though weak brines are frequently strengthened for manufacturing purposes by adding rock salt. Their composition depends on the peculiar nature of the original source, and on the secondary influences to which they are exposed in passing to the surface; an intercepting stratum often modifying materially their original composition.† Moreover brines from the same salt deposit frequently differ widely in composition; the upper parts of the deposit containing as a general rule, more of the foreign salts than the lower portion; the exceptions being accounted for by extensive surface erosions and percolating waters.‡ Again, the upper layers of salt deposits being more exposed to surface action, as for instance, to infiltrations, which may cause chemical changes, are thus particularly liable to suffer from the accumulation of foreign admixtures, derived from the disintegration of the overlying rocks. Brines originating from the surface layers of a salt deposit are therefore usually inferior in composition to those coming from its lower portion.

The impurities of brines are those which are found in rock salt, but in many instances they contain also the carbonates of lime, magnesia, and protoxyd of iron, carbonic acid, and hydrosulphuric acid, besides organic matters. These last named have found access to the brine in most cases during its passage to the surface.

Brines are divided§ according to the character of the saline admixture, into two classes; those of the *first class* contain the

* See my statement in a report of the American Bureau of Mines. On the rock salt deposit of Petit Anse, etc., New York, 1867; and also Dr. E. W. Hilgard's statements in Proceedings of American Assoc. for the Advancement of Science, Aug., 1868.

† See my contribution to the Chemistry of Mineral waters, etc. Syracuse, N. Y., Feb., 1866, also this Jour., 1866.

‡ See my contribution to the Chemistry of Brines, this Jour., July, 1867.

§ Ibid., p. 80.

chlorids of calcium and magnesium, and sulphate of lime; those of the *second class* contain no chlorid of calcium, but only chlorid of magnesium, with the sulphates of lime, soda and magnesia; the first class are supposed to originate chiefly from the saline residue of the oceanic waters of ante-tertiary date, while the second class represent most probably derivatives of the former. All the brines found east of the Mississippi river belong, so far as my own information extends, to the first class, as they contain chlorid of calcium. The second class of brines is largely represented west of that river; for instance in Nebraska, Kansas, and Arkansas.

I. 1*st Class of brines and salt.**

	Brine of Cohoes, C. W.	Brine of Onondaga, N. Y.	Brine of Mich. (Saginaw Valley.)		Salt of Ohio, (Hocking valley.)		Brine of Kentucky.	Brine of Tenn
			I.	II.	I.	II.		
Sulphate of lime,	0·5433	0·5747	0·0755	0·4887	0·0881	0·1060	traces	0·2978
Chlorid of calcium,	0·0216	0·1533	2·9422	0·4020	1·1360	0·6140	0·7009	1·1793
Chl'd of magnes'm,	0·0336	0·1440	1·2616	0·3710	0·4744	0·0409	0·7312	0·9587
" of sodium, etc.	24·1433	15·5317	19·8595	12·5315	92·8	95·7	6·7684	9·8952
Water,	74·8	83·	76	86·0	5·9	3·4	91·	87·

II. 2*d Class of brines and salt*; it includes all kinds of salt made from oceanic waters.*

	Nebraska (brine),	Nebraska (salt).	Kansas (salt).
Sulphate of lime,	0·1256	0·2475	1·1222
Sulphate of soda,	0·5808	0·3912	0·3511
" " magnesia,	----	----	0·1794
Chlorid of magnesium,	0·1542	0·0790	0·2400
Chlorid of sodium, etc.,	0·3150	98·12	93·06
Water,	90·	0·8	4·8

The value of a brine for manufacturing purposes does not depend entirely either upon its density or upon the relative percentage of chlorid of sodium and of foreign saline admixtures; the *kind* of the impurity is of the greatest importance; for instance, the sulphate of lime and even the sulphate of soda are considered far less objectionable, within proper limits, than a corresponding amount of the deliquescent chlorids of calcium and magnesium.

Saline springs are scattered all over the United States, their number having been largely increased by the recent extensive explorations for oil; the amount of brine which they can yield is apparently inexhaustible. Foremost among them are the brines of New York, (Onondaga co.), southeastern Ohio, western Virginia, Michigan, Pennsylvania, and of late Nebraska and Kansas. The brines of New York, Ohio, Virginia, Pennsylvania, Michigan, Tennessee, Kentucky, and Canada West, resemble each

* These analytical statements by no means express the commercial value and the relative purity of the salts and brines, they claim merely to indicate their peculiar character.

other closely. They all belong to the first class of brines, and all contain chlorid of calcium; they differ merely in the relative proportion of the impurities which they contain. The brines of Lincoln co., Nebraska, and of Smoky Hill river valley, Kansas, etc., contain chlorid of magnesium, and the sulphates of lime and soda, but no chlorid of calcium. They represent the second class of brines. Most of the saline waters of this country are, even in their natural state, strong enough to be worked directly by artificial heat. All our home manufactured salt, coarse as well as fine, with the exception of a small quantity obtained from sea-water, has thus far been made from natural brines; fully one half of the whole amount for a number of years having been obtained from the brines of Onondaga, New York State.

C. *Sea-water.*—The water of the ocean is a weak brine; it contains from three and one half to four per cent of saline matter, of which three fourths is chlorid of sodium and one fourth is impurities; it is free from chlorid of calcium, and belongs properly to the second class of natural brines. Its impurities consist mainly of chlorid of magnesium, the sulphates of lime, magnesia and soda, besides smaller quantities of chlorid of potassium, bromid and iodid of magnesium, and carbonate of lime, with traces of oxyd of iron, etc. Sea-water varies but little in composition and concentration, except in localities where either a limited body of water is prevented from unrestricted communication with the ocean, or where a large influx of fresh water causes its dilution. It represents the main source of supply for the manufacture of salt in France, Portugal, Spain, Italy, the West Indies, and Central and South America; it is used also largely for the production of salt in England, Belgium and Holland, being frequently employed for the solution of rock salt of inferior color. In the United States it has been turned to advantage but to a very limited extent. Three hundred to three hundred and fifty thousand bushels cover, in all probability, our present production of salt from sea-water. The States of Massachusetts, North Carolina and Florida on the Atlantic, and of California on the Pacific Ocean, have mainly been interested in this branch of industry. The late increase in the production in Florida, and on the Pacific coast, more than compensates for the falling off elsewhere.

In concluding this paper it may be well to re-state the fact, that the comparative commercial value of various samples of salt, so far as their composition is concerned, does not always depend upon the relative proportion of chlorid of sodium to foreign admixture. A mere analytical statement giving the percentage of impurity, without specifying its kind and nature, gives no reliable standard by which to pronounce upon their commercial

value, since the various impurities, which the salt has retained, are objectionable in quite different degrees; thus a sample which contains from one to one and a half per cent of foreign salts, consisting almost exclusively of sulphate of lime, may claim to be a very fair article of common salt; whilst if it contains but one half of that amount of the chlorids of calcium and magnesium or of the sulphates of soda and magnesia, together with the chlorid of magnesium, or of the carbonates of lime and magnesia, it would be considered quite objectionable, at least for table and dairy purposes. Again, the superior fitness of a sample of salt for many domestic purposes, does not depend entirely on its chemical composition; its mechanical condition is quite frequently of not less importance. A well devised mode of manufacture, ought therefore, not only to aim at the most perfect removal of foreign impurities, it must also secure to the salt the best mechanical condition. The manufacturer in selecting a mode of working is consequently limited in his choice; he must often sacrifice purity to mechanical condition, being obliged to adopt a method which promises to secure most economically a desirable article from his brine in the purest condition possible. So far as the form is concerned there are two kinds of salt in commerce; (a), *coarse salt* including salt made by solar heat, and rock salt crushed to a suitable size; and (b), *common fine salt*, or boiled salt obtained by artificial heat and more rapid evaporation; both kinds have their special market value. The amount of salt consumed for the promotion of animal life far exceeds that required by vegetables, but the amount demanded for meat packing and for the dairy business is very much larger. In the industrial arts, we can scarcely claim that any of our home resources have been turned to account. A proper exposition of the various uses to which salt is applied in chemical manufacturing industry would be a description of one of the most important chemical arts of the present day. To do justice to this question would require more time than I feel entitled to claim. I shall therefore confine myself merely to the presentation of a few statistics in regard to the quantity annually produced.

England produces from thirty to thirty-two millions of bushels of salt, (56 lbs. each), of which seventeen millions are used for the manufacture of soda ash, sal soda, caustic soda, bicarbonate of soda, hydrochloric acid, bleaching powder, etc., etc., while about two-thirds of the rest is exported, leaving scarcely one-fifth of its whole production for domestic consumption.* The production of salt in the United States amounts at

* England abolished its tax on salt in 1823. The price has varied only within narrow limits for many years past; the production in some localities has changed much since that time; for instance, Worcestershire produced in 1823 but 9,000

present to from sixteen to eighteen millions of bushels, and the consumption to from thirty-two to thirty-four millions of bushels; in other words, almost one bushel to every head of its population. This large consumption of salt is due to our extensive meat packing and dairy business; the consumption for manufacturing purposes being scarcely worth mentioning. Almost one-half of the amount of our present consumption, it will be noticed, is imported. We must consequently increase our production to twice the present amount, if we would supply our present demand; and if we would support the industrial arts at the rate England does, we must increase this production more than three times. Our natural sources of supply would, even in their present incomplete state of development, suffice to furnish the amount required. Our prospects might therefore be considered highly gratifying, if the question of quantity only were involved. But as the cheapness of the raw material and of labor are not less important points, which ought to be duly weighed before entering upon a new industrial enterprise in which skill and capital have for years secured a monopoly in the market, we must acknowledge that we are not yet fully prepared to enter the lists as successful competitors. Some of our older salines are not yet sufficiently explored to warrant the expectation of a cheap supply from them in their present condition, and many of our recently discovered brines are too far from cheap communication or from centers of skill and industry, to be to any extent available for our present emergencies.

In some of the countries in Europe, where the government holds the salt monopoly for revenue purposes, the practice has for obvious reasons been adopted of taxing the salt used for domestic purposes, while the salt used for manufacturing purposes is supplied at cost, being first rendered unfit for domestic application by the addition of ground charcoal, oxyd of iron, etc. These additions are selected with reference to the particular use for which the salt is intended. May intelligently directed individual enterprise, supported by a wise legislation, soon recognize the proper means by which our home production may be best stimulated, and thus our chemical industry receive the most important element for its successful and rapid development.

tons, while its present production is 200,000 tons; the Cheshire salt works are capable of making one million of tons per year; in both cases the sources of supply exceed the demand. The Worcestershire works export annually 50,000 tons, while the Cheshire works export 650,000 tons. The Stoke's works employ 500 hands, produce about 3,000 tons per week, consuming 1,500 to 2,000 tons of coal. A fair workman at 2s. per ton will make 28s. per week.—(*Chem. News*, No. 377, Feb., 1867, page 88.

Amherst, Mass., August, 1869.

ART. XII.—*Account of a fall of Meteoric Stones near Danville, Ala., with an analysis of the same;* by J. LAWRENCE SMITH, Louisville, Ky.

ALTHOUGH the meteorite of Danville, Alabama, fell in Nov., 1868, and an analysis has been made of it during the past summer, it is only recently that I obtained a complete account of the phenomena attending its fall.

On Friday evening, Nov. 27th, 1868, about five o'clock, Mr. T. F. Freeman, of Danville, (about lat. 34° 30′ and long. 87° W. Greenwich), on stepping from his house, was startled by a loud report, so much like artillery that for the moment its origin was attributed to the firing of a small piece of artillery kept in the village, but on inquiry it was ascertained that no firing had taken place there, but that the sound was heard at the village, and attributed to very heavy artillery at Decatur, Trinity, Hillsboro, or some other point to the northward of Danville. During the war, artillery had been often heard in the valley of the Tennessee, and various speculations were indulged in as to what was meant by this cannonade at such a time of day and in such a direction.

The following day, Mr. Wm. Brown, living three miles west of Danville, brought to the village a piece of rock which he said fell near him and some laborers, who were picking cotton. He dug it up at a depth of about 1½ to 2 feet. It weighed about 4½ lbs., and had the characteristic aspects of a meteoric stone; but it was broken by the party obtaining it, and all but about half a pound, now in my possession, has been scattered and probably lost or thrown away.

Several other stones fell in the same vicinity. Some negroes working in a cotton field on the plantation of Capt. McDaniel, half a mile from Danville, heard a body fall with a whizzing, humming sound, and strike the ground near them with tremendous force; but they were alarmed and did not approach the spot that night; a rain fell during the night and no trace of it could be found the next day. Various other stones were heard to fall in different parts of the adjacent country. Two brothers, by the name of Wallace, were ploughing in their field, about 1¾ miles N. W. of Danville; they distinctly heard two or three fainter reports, after the first loud one, and heard the sound of two falling bodies whizzing down, one to the right and the other to the left of them.

With the above data, and the known geography of the country, its direction must have been N. E. and S. W., but it is impossible to say from which of these quarters it came.

The portion of the meteorite that I possess has a large portion of it covered with the usual black crust. Its general aspect is rough and dull; a portion of the outer surface, not covered with the black coating, is nevertheless a surface that it had when it reached the ground, for on this surface are streaks and little patches of a bright, pitchy matter, which was once fused, and was derived either from another part of the coating that was thrown off in a melted state from the coated portion, and whipped around, (as it were), on to the unfused surface as the stone fell through the air, or from an incipient fusion that was begun on the denuded surface, and arrested by the termination of the fall. Where the black crust reaches the denuded places, it appears to be rounded off, as if it had been melted matter passing from another portion of the stone, and rolled over the surface of the borders.

The broken surface has a dark gray color, and is somewhat oolitic in structure, but not as much so as many other meteoric stones. There are veins and patches of a slate-colored mineral running through it. Pyrites and iron are also to be seen diffused through the stone; thin flakes of the iron giving that slickenside-like appearance to a fracture not unfrequently seen in this class of bodies. There seems to be more of iron in the slate-colored mineral than in the other parts. There are a few patches of white mineral, which I take to be enstatite. The specific gravity of the stone is 3·398.

For further examination, a portion of the meteorite was separated mechanically into three parts; the pyrites, the metallic iron, and the earthy minerals. As in the case of most meteorites, the earthy minerals were so intermixed that it was impossible to separate the different varieties, three of which were easily traceable by the eye.

The iron separated with great care from the pulverized meteorite constitutes 3·092 per cent of the entire mass, and an analysis furnished

Iron,	89·513
Nickel,	9·050
Cobalt,	0·521
Copper,	minute quantity
Phosphorus,	0·019
Sulphur,	0·105
	99·208

The sulphid of iron detached very carefully from the mass of the meteorite gave

Iron,	61·11
Sulphur,	39·56
	100·67

which corresponds with the protosulphid of iron, FeS. Whether it contains any of the sulphid known as troilite I am not prepared to say.

The stony minerals were freed as much as possible from iron and pyrites, and one gram treated with ten grams of hydrochloric acid on a water bath, and evaporated nearly to dryness, then filtered and the filtrate well washed ; after which, the residue in the filter was warmed with a solution of caustic soda to dissolve any silica belonging to the portion dissolved by the acid ; it was then filtered again and washed. The result was

Soluble portion	60·88
Insoluble portion	39·12

The treatment by a solution of caustic soda or potash is of importance for a correct result, as otherwise a portion of the silica of the decomposed minerals will be estimated with the portion that is undecomposed.

The insoluble portion was analyzed ; for although the analysis made in this way cannot furnish any positive indication in regard to the true mineral constitution of the meteorite, it is, nevertheless, an important guide. It was found to consist of

Silica	50·08
Alumina	4·11
Protoxyd of iron	19·85
Magnesia	20·14
Lime	3·90
	98·08

From all the circumstances connected with this mineral, its physical characters, &c., it is doubtless a pyroxene of the augite variety.

The soluble portion, owing to the unavoidable presence of a little iron and pyrites, simply furnished results on analysis that showed it to be mostly olivine. The only matter, as a whole, freed as much as possible from pyrites and nickeliferous iron, gave

Silica	45·90
Protoxyd of iron	23·64
Magnesia	26·52
Alumina	1·73
Lime	2·31
Soda	·51
Potash	·64
Oxyd of manganese, a minute quantity, not estimated.	
Oxyd of Chrome, " " " "	
Phosphorus, " " " "	
Lithia—marked reaction with the spectroscope.	
Sulphur	1·01

The excess in the footing up of the analyses above 100 per cent is due to the fact that a part of the iron, estimated as protoxyd, is combined with sulphur forming sulphid of iron.

This meteoric stone is similar in every respect to that which fell March 28th, 1859, in Harrison county, Indiana, (which locality I see referred to, in catalogues of meteorites, as Harrison county, Kentucky). This meteorite is therefore composed of

Nickeliferous Iron,.....
Olivine,.....
Pyroxene,.....
Protosulphid of iron,.....

with minute quantities of schreibersite, chrome iron, and probably albite.

In concluding these observations on the Danville meteorite, I cannot but feel more and more convinced of the importance of a thorough reëxamination of the mineral nature of the meteoric stones, and in the present case, I am not at all satisfied that the mineral characteristics are perfectly made out.

ART. XIII.—*Contributions to Zoölogy from the Museum of Yale College.* No. V.—*Descriptions of Echinoderms and Corals from the Gulf of California;* by A. E. VERRILL.

THE Museum has recently received a large and important collection of Radiata, collected by Capt. J. Pedersen in the vicinity of La Paz. The following notes and descriptions relate to some of the more interesting species only.

ECHINOIDEA.

Meoma nigra Verrill.

Meoma nigra Verrill, Trans. Connecticut Acad., i, p. 317, 1867.
Kleinia nigra A. Agassiz, Bulletin Mus. Comp. Zoöl., p. 27, 1863.

Of this interesting species there are ten specimens in the collection, which show considerable variation from the type formerly described by me, as well as among themselves.

The largest is 4·85 inches long, 4·25 broad, 2·10 high; the smallest 3·85 long, 3·40 broad, 1·75 high. The outline, as seen from below, varies but little and is broad-oval, somewhat emarginate anteriorly, obliquely truncate posteriorly, and slightly compressed laterally, or, in other words, nearly heart-shaped. The anal area is large, somewhat sunken, and is at the extreme posterior end of the shell, occupying the greater part of the truncated portion. Its form varies from regularly elliptical, acute at each end, to broad-oval, rounded below and acute

above; its position varies from nearly vertical to decidedly oblique, and it is so nearly terminal as to produce a posterior emargination in a dorsal view of the shell. In a side view some specimens are decidedly depressed, but most are regularly arched, while one is decidedly elevated at the apex. There is considerable variation in the depth of the anterior ambulacral groove, and also in the number and prominence of the large tubercles, which are more or less restricted to the region enclosed by the peripetalous fasciole. The fasciole itself shows remarkable variations, but does not agree at all with that of *M. grandis*, as figured by Gray. The portion crossing the anterior interambulacral regions varies less than other parts, but in some the intermediate transverse portion is nearly straight, in others strongly curved and often crooked, in one it is bent up into a right angle on each side of the ambulacral groove; its bend or angle near the antero-lateral grooves is also variable, both in form and extent, it being twice as large in some specimens as in others, and in one an irregular, crooked branch passes from the apex of the angle on the left side to the anterior groove. In the posterior interambulacrum the course of the fasciole is quite variable, in five examples it crosses with a strongly curved upward bend, without any distinct angle, rising highest in four specimens on the right side, in the other forming a nearly straight transverse middle portion; in three specimens it forms a sharp angle on the right side; in one a similar angle on the left side; in another there is a strong median angle, its apex pointing to the anal region, and another to the right of it, pointing to the summit; in all the specimens it bends inward further than in *M. grandis*. The lateral part of the fasciole also varies, especially on the left side; in five (but not the same five that agree in the posterior region) it has but one angle, near the antero-lateral grooves, where it rises highest; in three it has two angles, rising highest at the posterior one, and nearly straight between; in two others, which also have two angles, the transverse part is double. On the other side the fasciole varies in the same way, but not in the same specimens, for some have two angles, both on the right and left; others two only on one side; others one on both sides. The anal fasciole is also variable; usually the subanal branch is wanting or indistinct, though indicated by a band of smaller tubercles, but in one specimen it is well marked and the subanal disk is clearly and perfectly circumscribed. In this the subanal disk is very broad, bilobed, narrowest in the middle, scarcely heart-shaped, the anterior border being nearly transverse, and the posterior border nearly parallel with the anal region and about ·15 of an inch from it. In others the posterior border is more curved. One specimen has but three ovarial openings, the rest four. The

proportionate length of the ambulacral grooves varies considerably, both in different specimens and on opposite sides of the same individual, sometimes those on the right being longest, sometimes those on the left, and not uncommonly a longer anterior one is offset by a shorter posterior one on the same side.

Mr. A. Agassiz thinks this species may be identical with *M. grandis* Gray, described as from Australia, but if the figure of the last be at all correct my specimens differ widely from that described by Gray. The position of the anal area, especially, is quite different, it being in Gray's figure at a considerable distance from the posterior end, and therefore more ventral and nearer the subanal fasciole. The peripetalous fasciole is also very different from that of any of my specimens.

Agassizia subrotunda Gray.

Catalogue Ech. of British Mus., p. 63, tab. 3, fig. 2; Verrill, Proc. Bost. Soc., vol. xii, p. 381.

A. ovulum Lutken, Vidensk. Medd., p. 134, tab. 2, fig. 8; Verrill, Trans. Conn. Acad., vol. i, p. 320.

Of this species there were about a dozen specimens, mostly more or less broken, which show but little variation and agree well with Gray's figure.

Mr. A. Agassiz regards this and *A. ovulum* Lutk. as identical with *A. scrobiculata* Val., which may well be the case if the figures in the Voyage de la Vénus be incorrect, as he states. The figures are certainly very unlike our specimens.

Clypeaster speciosus Verrill, sp. nov.

Depressed, gradually rising toward the apex; the lower side sometimes slightly concave from near the edge of the mouth, in other specimens flat except close to the mouth, which is much sunken. Outline oblong-pentagonal, with rounded angles and slightly concave sides. The anterior end slightly elongated. Interambulacral regions decidedly concave between the ends of the ambulacral rosette; the ambulacral regions enclosed by the pores slightly raised, narrow, elongated, widening but little outwardly and somewhat acuminate at the end, which is often nearly enclosed by the pores. The interambulacra are broader and decrease much more rapidly toward the apex than in *C. rosaceus*. Anal opening tranvsersely oval, or rounded, situated about its own diameter from the edge of the shell.

Length of largest specimens 4·60 inches; breadth 3·90; height 1·15. Length of anterior petal, from the apex 1·90, its breadth ·82, breadth of enclosed space ·50; length of anterior petals 1·70, breadth ·85, breadth of enclosed space ·48; length of posterior petals 1·80 and 1·85, breadth ·94, of enclosed space ·58 and ·60; diameter of anal opening ·20; of actinal opening ·33.

Thirty-five specimens of this species are contained in the collection. They show but little variation in outline, except what

is due to age, though some specimens are more elevated toward the apex than others; in regard to the flatness or concavity of the lower side there is, however, great variation, though Dr. Gray used this character in dividing the genus into sections. The youngest specimens are 2·30 long by 2·10 wide, and are more oval in form and scarcely angular, but have the flatness and form of ambulacral rosette characteristic of the larger specimens, as well as the same position of the anal opening.

From *C. rosaceus* of the Atlantic this species differs widely, the former having a much more elevated and thick form, with broader and more obovate ambulacra, which are much more swollen; the lower side is much more concave, and the anal opening nearer the edge.

Some of the species described by Dr. Gray from the Indo-Pacific faunæ (*C. Australasia*, *C. testudinarius*) seem to be more closely allied, but only a direct comparison of specimens can settle the true relations of these species. It may be that *C. testudinarius* is the same and its locality incorrect, its outline being nearly identical, but the upper side is said to be evenly convex, and the lower side concave from the margin.

This species is of especial interest as the first of the genus known from the Pacific coast of America, although the genus was known to occur on nearly all other tropical coasts.

Encope grandis Agassiz.

Of this very distinct species there are numerous specimens in the collection, varying in size from 3 inches in length by 3·20 wide, to 4·60 inches by 4·40. There is but little variation in outline and general appearance, and in all the margin is thick, with the five large notches widely open, though in the larger there appears to be a tendency to close the anterior pair. The posterior interambulacral opening is large and broad-oval with thickened borders in all, but there is a variation of more than 50 per cent in its relative size; the region around it is in all more elevated than the central region and considerably swollen. The form of the ambulacral rosette varies considerably. The three anterior petals are subequal and usually long-oval, obtusely rounded at the end, but in one case they are narrower and more elliptical, especially the odd anterior one, which is widest in the middle, tapering to each end, and in another they are broader and more dilated outwardly than usual; the two posterior ones are much longer, widest outwardly, and curve somewhat around the posterior opening, but they vary considerably in relative width. The following are the proportions in two extreme specimens:

From abactinal center to posterior edge,	2·20	2·20
Center to anterior edge,	1·98	2·00
Center to lateral edge,	2·20	2·10
Length of anterior odd ambulacral petal, from center,	1·28	1·25
Greatest breadth of do.,	·50	·68
Breadth of its enclosed area,	·20	·30
Length of antero-lateral pair,	1·25	1·15
Breadth of do.,	·50	·65
Breadth of enclosed area,	·16	·27
Length of posterior pair,	1·65	1·55
Breadth of do.,	·45	·62
Breadth of enclosed area,	·12	·20

The branchings of the ambulacral grooves beneath are quite constant in their arrangement, but the relative breadth and form of the enclosed areas are quite as variable as in the dorsal rosette. The region about the anal opening and around the posterior foramen is sometimes deeply concave or excavated; in most cases slightly so; and sometimes not at all.

Encope Californica Verrill, sp. nov.

Test broad, thin at the edge, rounded anteriorly, broadest behind the middle, sub-truncate or rounded posteriorly; usually about as broad as long, sometimes broader than long. Apex behind the center. In profile the outline descends from the center to the anterior edge, but rises from the center to the posterior foramen, from which it descends rapidly to the edge. The posterior interambulacrum is, therefore, swollen and the test is most elevated near its foramen. Ambulacral rosette with the petals long-oval, somewhat obovate, broadly rounded outwardly; the anterior pair shortest and most rounded; the odd anterior one somewhat longer and narrower and a little shorter than those of the posterior pair, which are of about the same form and not curved. Posterior foramen variable in form and size, usually rather small, regularly oval, or rounded, sometimes long oval, or even narrow and elongated, occasionally quite large and broad oval, often obovate beneath, sometimes constricted in the middle. Ambulacral foramina also quite variable in form and size, but commonly small and rather regularly oval, often at a considerable distance from the margin.

Two specimens, showing the extreme variations, give the following measurements:

Length of test,	4·75	4·30
Breadth,	4·65	4·30
Center to anterior edge,	2·45	2·15
" " anterior foramen,	1·80	1·65
" " lateral edge,	2·35	2·15
" " " foramen,	1·60	1·50
" " posterior edge,	2·45	2·25
" " postero-lateral foramen,	1·85	1·70
" " posterior foramen,	1·15	1·10
Length of " "	·67	·60

Breadth of posterior foramen,	·22	·26
Length of anterior ambulacral petal, from center,	1·42	1·32
Breadth where widest,	·65	·50
Breadth of enclosed area,	·30	·18
Length of antero-lateral petals,	1·28	1·10
Breadth,	·67	·50
Breadth of enclosed area,	·28	·20
Length of postero-lateral petals,	1·58	1·35
Breadth,	·68	·53
Breadth of enclosed area,	·25	·16

Of this species there are 74 specimens in the collection from La Paz, and I have seen others from Cape St. Lucas. It varies considerably in outline, and in the form of the openings, especially the posterior one; the ambulacral rosette varies somewhat in the form of the petals, as shown by the above measurements; the ambulacral grooves beneath also vary in direction. But all the specimens agree in having their greatest elevation behind the center, or the posterior interambulacral region swollen. This peculiarity, which is found to depend upon a very different internal structure, will readily separate this species from *E. occidentalis* V., and from *E. micropora* Ag., whether those be the same, as Mr. A. Agassiz supposes, or not.* In *E. occidentalis* the greatest elevation is in front of the center, and there is a regular slope from thence to the broad, thin, posterior edge, and the sections show that the wide space between the central cavity and the posterior foramen is filled with a pretty firm, alveolary tissue having comparatively small spaces, but in *E. Californica* the same region is much less extensive (owing to the relatively larger central cavity and jaws) and is filled with a much less firm and more open tissue, with large cavities.

The difference is therefore analogous, in respect to external form at least, to that which separates *E. Michelini* from *E. emar-*

* Mr. A. Agassiz, in the Bulletin of the Museum of Comp. Zoölogy, No. 9, p. 266, 1869, says: "from a careful comparison of specimens of *E. cyclopora*, *micropora*, and *perspectiva* (Ag. species) there is no doubt that these are only nominal species, all identical with Verrill's *E. occidentalis*." The latter is the same as *E. tetrapora* Ag. (non Gmelin), but I believe Mr. Agassiz goes too far in uniting all these other forms into one species, for if this can be done there is no reason whatever for keeping the species from the two coasts separate, for there is often much less difference than between some of those forms, which he has united. But since the localities of the types of *E. micropora*, *E. cyclopora*, and *E. perspectiva* were unknown, the two last having been described from unique specimens, it seems to me more probable that some of them represent imperfectly known species from other regions; for such species have been recorded from Africa and other little explored localities. Mr. Agassiz does not state what he regards as the characteristic of the polymorphous species thus constituted, nor in what respect it differs from *E. emarginata* of the Atlantic, nor does it appear certain that he has examined authentic specimens of all these forms. Dr. Lutken referred both *E. micropora* and *E. grandis* to the Atlantic *E. emarginata*, to which he also united *E. Valenciennesii* Ag., *subclausa* Ag., *oblonga* Ag., *quinqueloba* Esch., and *E. Michelini* Ag. In regard to *E. grandis* and *E. Michelini* he appears to have erred, but the figures of *E. micropora* resemble some forms of *E. emarginata* more than *E. occidentalis*, from which *E. cyclopora* and *E. perspectiva* differ still more widely.

ginata,—a distinction which Mr. A. Agassiz admits as valid in that case, where there is, however, much less difference in the form of the rosette and other characters.

The only figure in the monograph by Prof. Agassiz, which approaches this species, is that of *E. emarginata* (Tab. 10), which has, to some extent, the same posterior elevation. The specimen figured was from an unknown locality, and may possibly represent a variety of this species rather than of the *E. emarginata* of Lutken, A. Agassiz, etc., which is common on the Atlantic side of tropical America.

Astropyga venusta Verrill, op. cit., p. 296.

Two fine large specimens of this rare species are in the collection.

The largest is 5·80 inches broad and 2·10 high. It was previously known to me only from Panama Bay and San Salvador.

Tripneustes depressus A. Ag.; Verrill, op. cit., p. 375.

Of this large species there were 19 specimens, with their spines partially preserved. They are quite variable in form, but mostly even more elevated than ordinary specimens of *T. ventricosus*. Some are conical, others broadly rounded above. The name, therefore, is a decided misnomer. The largest spines on the upper surface of the largest specimen are ·45 of an inch long, ·04 in diameter, and rapidly taper to the acute point; those of the lower surface are often ·60 of an inch long, ·04 in diameter, tapering but little, the end blunt.

Several specimens give the following proportions:

Diameter, (inches)	5·80	5·40	5·35	5·25	5·10	4·90	4·75	4·60
Height,	3·00	3·40	2·90	3·25	2·85	2·65	2·85	2·80

ASTROIDEA.

Pentaceros occidentalis Verrill.

Oreaster occidentalis Verrill, op. cit., p. 278, 1866.

Of this hitherto rare species there are 21 specimens of various sizes. They show but little variation except that due to age or state of preservation. Some specimens are so dried as to leave the disk and rays plump and rounded above, while in others the interradial spaces are so shrunken as to make both the rays and disk angular. In some most of the upper and part of the lower marginal plates bear small obtuse spines or tubercles; in others there are few or none of these; the two smallest specimens have none, though others scarcely larger, have quite a number. The smallest specimen has the longer radius 1 inch; the shorter ·50. This, however, has nearly the form and all the

essential characters of the adult, though the spines and tubercles are less numerous.

The name of the genus, *Pentaceros* Gray, has priority over *Oreaster* M. & Tr., which was substituted on the ground that the former was in use among fishes,* which I have been able to confirm, although Mr. A. Agassiz adopts it.

Among the other starfishes are *Gymnasteria spinosa* Gray (large); *Amphiaster insignis* Verrill, *Nidorellia armata* Gray, *Ophidiaster pyrimidatus* Gray, *Linckia bifascialis* Gray, *Acanthaster Ellisii* Verrill, *Mithrodia Bradleyi* Verrill, etc.

Corals.

Fungia elegans Verrill, sp. nov.

Coral when young regular and round, often becoming slightly oval; when adult usually more or less angular, the edge plicated, forming six to twelve lobes. The upper surface becomes very convex in mature specimens and the lower surface deeply concave and covered with very numerous, fine, subequal, elevated costæ, which are finely dentate on the outer half, becoming nearly entire and very faint toward the center, which usually shows the scar where attached when young. Septa thick and rather crowded, very unequal, the six primaries very prominent and thick at the inner end; those of succeeding cycles successively shorter and less elevated. Edges of septa unevenly crenulate, or finely dentate. Columella slightly developed, loosely spongy; median fosette small, narrow, elongated, the two septa in the direction of its longer diameter much less elevated and thinner than the rest. Trabiculæ stout, conspicuous, often coalescing into continuous transverse plates.

The smallest unattached specimens are ·90 of an inch broad by ·35 high; ordinary specimens are 1·90 broad by 1·10 high; the largest 2·35 by 1·20.

This is a small and very distinct species, remarkable as the first one of the genus described from America.

Among the other corals are *Pocillipora capitata* V., *P. capitata, var. porosa* V., *Cœnopsammia tenuilamellosa* E. & H., *Porites* (two species), *Stephanaria stellata* V., *Callipodium Pacificum* V., *Eugorgia aurantiaca* V., *E. nobilis, var. excelsa* V., *Leptogorgia rigida* V., *L. Agassizii* V., *L. media* V., *Muricea austera* V., *M. appressa* V., etc. Most of these are represented by numerous large and beautiful specimens.

* Cuvier and Val., vol. iii, p. 30, 1828; Gunther, Catal. Fishes British Museum, i, p. 212.

ART. XIV.—*Note on the Generic relations and Synonymy of the Common Sea-urchin of New England* (*Euryechinus Drobachiensis* Verrill); by A. E. VERRILL.

THIS common sea-urchin, which is found upon the northern coasts of Europe, and both upon the Atlantic and Pacific coasts of northern America, was referred to the genus *Echinus* by all writers up to 1846, but under various specific names: *E. Dröbachiensis* Müll., *E. neglectus* Lam., *E. granularis* Say, *E. chlorocentrotus* Brandt, etc. Prof. Agassiz, in the introduction to his monograph of the Scutellidæ, p. 7, 1841, proposed several new subdivisions of *Echinus*, merely giving generic names and citing "les types." Among these was *Toxopneustes* with "*E. pileolus*" as its type. But afterward, in the introduction to Valentin's Anatomy of the genus *Echinus*, Dec., 1841, he revised the genus then known as *Echinus*, and made many judicious and necessary changes. At that time he separated and characterized the genera, *Temnopleurus*, *Tripneustes*, *Amblypneustes*, *Holopneustes*, *Stomopneustes*, and *Toxopneustes*.* For the last he here takes for type, the *Echinus tuberculatus*. The genus *Echinus* he restricted (p. ii) so as to include only "the species near *Echinus esculentus*, and which have, like it, the ambulacra formed of several arched series of pores disposed in pairs." To this group he referred all the European species, including not only *E. Melo*, *E. esculentus*, *E. Flemingii*, *E. granularis*, *E. brevispinosus*, *E. æquituberculatus*, *E. miliaris*, etc., but also *E. neglectus* (*Dröbachiensis*) *and E. lividus*, which now constitute my genus *Euryechinus*. It is plain, therefore, that at that time the genus *Toxopneustes* had nothing to do with such species as I have called *Euryechinus*. Later, in the Catalogue Raisonné, 1846, *Toxopneustes* (sub-genus) was extended so as to include, in addition to the typical species, several previously placed by Agassiz in *Echinus*. Still later Desor removed part of the species thus erroneously added, under the name of *Sphærechinus*, but he still left in it *Dröbachiensis* and *lividus*. Therefore as these were evidently generically distinct from the typical *Toxopneustes* and had received no generic name, I formerly proposed that of *Eurychinus*,† and gave my reasons in detail.

* The following is the description: Le genre *Toxopneustes* a des ambulacres formés de séries arquées de doubles pores, convergeant vers le milieu des aires et séparées par des rangées parallèles de petits pores. Chaque série arquée se compose de six à neuf paires de pores. Vers la bouche il y en a moins; mais elles sont plus rapprochées. Les tubercules des séries principales sont assez grands; l'ouverture inférieure du test offre dix échancrures peu profondes. Je prends pour type de ce genre l'*Echinus tuberculatus*; j'en connais quelques espèces inédites."

† Proc. Boston Society Nat. Hist., x, p. 341, 1866, and Transact. Connecticut Acad., i., p. 394, 1867.

To any unprejudiced person it will be evident from this statement that, as left by Desor after his latest restrictions, *Toxopneustes* included two generic groups, one of which was the original and typical *Toxopneustes* of 1841, the other an unnamed genus, (*Euryechinus*). The fact that Mr. A. Agassiz, apparently overlooking the true state of the case, renamed in 1862 the original *Toxopneustes* section, calling it *Toxocidaris*, cannot affect the matter in the least, since in this instance there can be no doubt as to which group was originally designated by the name, *Toxopneustes*. Yet Mr. Agassiz in a recent paper* goes considerably out of his way to bring up this matter again, and inserts the following note under *Echinometra Michelini*:

"I cannot see the propriety of the changes made by Verrill in the limitation of Toxopneustes, by substituting Euryechinus for a group of Echini, which are perfectly well known by all writers on Echinoderms as Toxopneustes. For the following reasons it seems to me, even granting all his premises, that the changes he proposes are not warranted. The type of the genus at the time the Monog. d. Echinides was written was never used in the restricted sense now common, but was coextensive with a group of species. When Toxopneustes was first proposed, it was applied to a so-called typical species which future investigations showed did not belong to the genus. The author took the earliest opportunity possible to point out his mistake by substituting for it another type, and giving a description which *applies not only to Toxocidaris as Mr. Verrill would have it, but also to all the species since removed as Sphærechinus by Desor.* Desor, who had edited the Catalogue Raisonné, and probably knew accurately what group of Echini was defined as Toxopneustes, was the first, in his Synopsis, to limit Toxopnuestes by removing from it certain species as Sphærechinus, and restrict Toxopneustes to such forms as (*T. neglectus*) *T. drobachiensis* Ag., but still including the species which I have since, in the Bulletin of the Museum, separated as Toxocidaris. All these limitations, even were they not accepted, have the priority over a similar limitation which Verrill makes twelve years after a proper limitation of the genus has been recognized, and eighteen years after a mistake (upon which Mr. Verrill bases the whole of his proposed changes) has been corrected by the author himself; nothing, moreover, is gained in accuracy by the change proposed by Verrill, *T. tuberculatus* being probably only a nominal species, and one concerning which we have, at any rate, no authentic information sufficient to form the basis of a sweeping reform. At the present rate of retrospective application of the laws of priority, we are fast drifting into the most absurd anachronism by applying the present condition of our knowledge of any group to works written twenty or thirty years ago in an entirely different spirit, when the idea of type, genera, etc., had a totally distinct signification from what it has at the present day."

A formal reply to this is perhaps unnecessary, yet I would venture the following remarks:

* Bulletin of the Museum of Comp. Zoölogy, No. 9, p. 260, November, 1869.

1. I am not aware that any other zoölogist has denied the validity of "type-species" (especially when particularly designated as such) in works even much earlier than 1841.

2. Even if the original description of *Toxopneustes* would include *Sphærechinus*, yet, as I have explained above, Agassiz himself referred the species of the latter to *Echinus* in the same article, *and he probably knew accurately what group was defined as Toxopneustes at that time.*

3. All the "limitations" by Desor, which are said to have priority over mine, I have always admitted and adopted.

4. I do not base any changes upon "a mistake," for whether "*E. pileolus*" or "*E. tuberculatus*" be taken as the type of *Toxopneustes*, the claims of *Euryechinus* remain unaffected, and I have, when making the change, distinctly stated my conviction that the reference to *E. pileolus* should not be regarded, and have adopted *Toxopneustes* in place of *Toxocidaris* A. Ag. (See Trans. Conn. Acad.).

5. Whether *E. tuberculatus* be "a nominal species" does not affect the character of the genus to which it belongs.

6. I know of no more fruitful source of confusion than the transferring of a name from the group to which it originally belonged to another totally distinct from it, and re-naming the first group.

7. Had Mr. Agassiz, before naming *Toxocidaris*, looked a little more closely into the early synonymy of *Toxopneustes*, all confusion in this case might have been avoided.

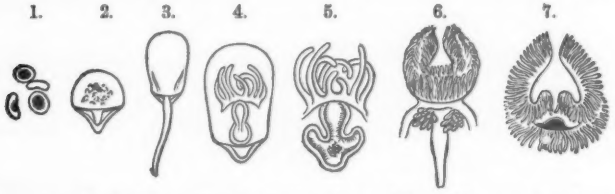
8. The fact that there is a *future* for zoölogical nomenclature, as well as a *past*, should not be forgotten, nor that a just and reasonable application of the universally recognized law of priority is the surest way of securing future stability.

ART. XV. — *On the Early Stages of Brachiopods;* by E. S. MORSE.*

THE writer made a visit to Eastport, Maine, early in the summer, for the purpose of discovering the early stages of a species of Brachiopod (*Terebratulina septentrionalis* Couth.) so abundant in those waters. As little has been known regarding the early stages of this class of animals the facts here presented will be of interest, as settling beyond a doubt their intimate relations with the Polyzoa. As the subject will be fully presented at the meeting of the American Association, only the more important features will be mentioned here. In a few individuals the ovaries were found partially filled with eggs.

* From the American Naturalist, September, 1869.

The eggs (fig. 1) were kidney-shaped, and resembled the statoblasts of Fredericella. No intermediate stages were seen between the eggs and the form represented in fig. 2. This



stage recalled in general proportions Megerlia or Argiope in being transversely oval, in having the hinge-margin wide and straight and in the large foramen. Between this stage and the next the shell elongates until we have a form remarkably like Lingula (fig. 3), having, like Lingula, a peduncle longer than the shell, by which it holds fast to the rock. It suggests also in its movements the nervously acting Pedicellina.

In this and the several succeeding stages, the mouth points directly backward (forward of authors), or away from the peduncular end (fig. 4), and is surrounded by a few ciliated cirri, which forcibly recall certain Polyzoa. The stomach and intestine form a simple chamber, alternating in their contractions and forcing the particles of food from one portion to the other. At this time also the brownish appearance of the walls of the stomach resembles the hepatic folds of the Polyzoa. Fig. 5 shows a more advanced stage, where a fold is seen on each side of the stomach; from this fold the complicated liver of the adult is developed, first, by a few diverticular appendages, as seen in fig. 6.

When the animal is about one-eighth of an inch in length the lophophore begins to assume the horse-shoe shaped form of Pectinatella and other high Polyzoa. The mouth at this stage (fig. 6) begins to turn toward the dorsal valve (ventral of authors), and as the central lobes of the lophophore begin to develop, the lateral arms are deflected, as in fig. 7. In these stages an epistome is very marked, and it was noticed that the end of the intestine was held to the mantle by attachment, as in the adult, reminding one of the *funiculus* in the *Phylactolæmata*. No traces of an anus were discovered, though many specimens were carefully examined under high powers for this purpose, the intestine of the adult being repeatedly ruptured under the compressor without showing any evidence of an anal aperture.

Art. XVI.—*On the existence of a Crocodile in Florida;* by Jeffries Wyman, M.D.

It has been shown by different paleontologists, especially by Dr. Leidy and Prof. Cope, that several species of Crocodilians existed in North America during the Cretaceous and Miocene periods, all of which became extinct. At the present time two living species of true Crocodiles, viz: *C. acutus* and *C. rhombifer*, are known in South America, and both range as far north as Cuba and San Domingo, but we have not been able to find a record of the presence of either of them within the limits of the United States, the Alligator being the only representative of the family to which it belongs.

While a guest of J. M. Forbes, Esq., on board the yacht Azalea, I had an opportunity of visiting Key Biscayne Bay in March, 1869, and while there Mr. William H. Hunt, of Miami, presented me the cranium here described. The animal to which it belonged, as I was told by the person who killed it, was shot near the mouth of the Miami river, which opens into the above mentioned bay. I was also informed that another had been killed in the same neighborhood.

The length of the head (from the alveoli of the incisors to the end of the occipital condyle) is 462mm, and the greatest breadth 191mm. The whole number of teeth is 68, viz: $\frac{1\,2}{1\,3}=\frac{1\,2}{1\,3}$; in the upper jaw the 4th and 10th, and in the lower the 4th and 11th are the longest. The first 12 teeth above and the first 11 below have the enamel fluted, and in both jaws the teeth behind these are marked with finely reticulated grooves. These hinder teeth are shorter and blunter, and thus more closely resemble the teeth of the alligator. The upper jaw is strongly convex and protuberant along the back of its middle portion.

When compared with a somewhat larger head of the *C. acutus* from South America (length 462mm, breadth 211mm), the Florida specimen closely agrees with it in the above as well as some other particulars. The following table shows some differences, however, relating chiefly to the proportion of the breadth to the length. In the table the length is assumed to be 1·000, and the measurements are in fractions of the length.

	South America.	Florida.
Greatest breadth of cranium	0·456	0·483
Length of palatine opening	0·214	0·210
Breadth " "	0·075	0·086
Breadth across pterygoids	0·300	0·313
Breadth at 4th tooth	0·162	0·169
Breadth at 10th tooth	0·218	0·235
Breadth at contraction behind 5th tooth	0·123	0·131
Breadth at contraction behind 12th tooth	0·190	0·210

In the South American specimen the markings of the surface of the bone consist chiefly of pits, while in the Florida specimen the markings are more in the form of grooves. In the first the parietal foramina are separated by a flat surface of bone 15 millimeters wide, while in the second there is only a blunt ridge. In this last also the lower jaw is somewhat longer, and its hinder part less recurved. The Florida specimen being the smallest is assumed to be the youngest; the differences of proportion above referred to may therefore depend upon age, as in young individuals the breadth is relatively greatest. In view of this we find no sufficient reason for considering the Florida specimen as belonging to any other than the sharp-nosed species (*C. acutus*). This conclusion is still further supported by a portion of the skin of the neck, which is preserved, and in which the nuchal plates are the same as in the species just named.

SCIENTIFIC INTELLIGENCE.

I. PHYSICS AND CHEMISTRY.

1. *On the emission and absorption of heat radiated at low temperatures.*—MAGNUS has communicated to the Royal Academy of Sciences in Berlin an important memoir on radiant heat, the principal results of which are, in the author's own words, as follows:

(1.) Different substances heated to 150° C. radiate different kinds of heat.

(2.) There are bodies which radiate only one kind of heat and others which radiate many kinds.

(3.) Perfectly pure rock salt belongs to the first class. Just as its ignited vapor or that of one of its constituents, sodium, sends out only one color, so this, even at 150° C., radiates only one kind of heat. It is monothermic, as its vapor is monochromatic.

(4.) Rock salt absorbs the heat radiated by rock salt in great quantity and more strongly than that of sylvin (KCl) and other kinds of heat. It therefore does not, as Melloni and Knoblauch suppose, permit all kinds of heat to pass through with equal facility.

(5.) The absorption of heat by rock salt increases with the thickness of the absorbing plate.

(6.) The great diathermancy of rock salt does not depend upon a small absorbing power for different kinds of heat, but upon the fact that it only radiates a single kind of heat, and consequently also absorbs only this, and that almost all other bodies at the temperature of 150° C. send out heat which contains only a small portion or none of the rays which are radiated by rock salt.

(7.) Sylvin behaves like rock salt but is not monothermic in an equal degree. In this also we trace the analogy with its own

ignited vapors or with those of potassium, which, as is well known, gives an almost continuous spectrum.

(8.) Fluorspar almost completely absorbs pure rock salt heat. We ought therefore to expect that the heat which it radiates should be strongly absorbed by rock salt. Nevertheless 70 per cent of it pass through a rock salt plate of 20^{mm} thickness. With reference to the sum of the heat which fluorspar radiates, which is more than three times greater than that from rock salt, this phenomenon may be explained, but the subject requires further investigation.

(9.) If it were possible to form a spectrum of the heat radiated at 150° C., this spectrum, if rock salt were the radiating body, would contain only one band. If sylvin were used for radiating, the spectrum would be more extended, but would still occupy only a small portion of the spectrum which would be produced by the heat radiated from lamp black.—*Pogg. Ann.*, cxxxviii, 333.

W. G.

2. *On the reflection of heat at the surface of fluorspar and other bodies.*—In a second paper, MAGNUS remarks that the newly discovered property of rock salt and other substances of sending out at 150° C. rays of but one or but few wave lengths makes it possible to institute experiments on the reflection of non-metallic surfaces The results of experiments have distinctly shown that such surfaces reflect different kinds of heat, or heat rays of different wave lengths, in very different degrees. A remarkable illustration is furnished by fluorspar. Of the heat radiated by different substances and received at an angle of 45°, quantities differing but little in amount are reflected by the following bodies:

Silver,	from	83 to 90	per cent.
Glass,	"	6 to 14	"
Rocksalt,	"	5 to 12	"
Fluorspar,	"	6 to 10	"

But fluorspar reflects 28–30 per cent of the heat from rock salt, while silver, glass and rock salt reflect no greater proportion of this than of other kinds of heat. Fluorspar reflects from 15 to 17 per cent of the heat from sylvin, which confirms the result deduced from the transmission of heat. To an eye which could distinguish the different wave lengths of heat, as well as the colors of light, fluorspar would appear brighter than other substances if all were illuminated at the same time by rays from rock salt. The same would be the case if rays from sylvin were incident, only in this case the fluorspar would be less bright. As the experiments of Magnus established the fact that even at 150° C. different substances send out very different kinds of heat, it follows that in every space an extraordinary number of different wave lengths are continually crossing each other. This variety of intersection is further greatly increased by the elective reflection which takes place at the different surfaces. An eye which could distinguish different wave lengths of heat, like the colors of light, would see all objects in the greatest variety of colors without any sensible heating of the objects themselves.—*Pogg. Ann.*, cxxxviii, 174. W. G.

3. *On the heat of the stars.*—Mr. HUGGINS has endeavored to determine whether a measurable amount of heat is radiated from the fixed stars. The incident rays were received upon the object-glass of a telescope of 8 inches aperture, in the focus of which was placed the surface of a thermo-electric pile consisting, for stars, of one or two pairs, for the moon, of 24 pairs. An astatic galvanometer of great delicacy was employed. The thermo-electric pile was enclosed in casings of pasteboard stuffed with cotton so as to exclude all changes of temperature as much as possible. The author describes the minute precautions taken, for which we must refer to his paper. The telescope could be directed to a star by means of the finder, the needle being at rest, and the image of the star kept by clockwork upon the face of the pile. It was found that the needle almost always began to move as soon as the image fell upon the pile, and that when the telescope was then directed to the sky near the star the needle usually began, after a minute or two, to return to its original position. From 12 to 20 observations were made upon the same star, and these observations repeated on other nights. In this manner observations of Sirius gave a deviation of the needle of 2°; those of Pollux 1½°. Castor gave no deviation; Regulus a deviation of 3° and Arcturus in 15 minutes also 3°.

Observations of the full moon did not give corresponding or reliable results. The results obtained with the stars are not strictly comparable, as it is uncertain whether the sensitiveness of the galvanometer was always the same.—*Proceedings of the Royal Society*, No. 109, 1869. W. G.

4. *On new sulphur salts.*—SCHNEIDER has discovered and described a remarkable series of sulphur salts which are formed by simply fusing together certain metals in the form of powder with sulphur and potassic carbonate. Sulphid of iron and potassium is formed by fusing together 1 part of pulverized iron, 6 parts of dry potassic carbonate and 6 parts of sulphur, until the mass has become uniformly and quietly fused. Water diffuses from the cooled mass much potassic sulphite and hyposulphite and leaves the new compound in crystals. The new salt has the formula $K_2S.Fe_2S_3$ or atomistically,

$$\begin{array}{c} S-K \\ | \\ S=Fe_2^{VI}=S \\ | \\ S-K \end{array}$$

It forms long flexible needles of brilliant luster and purple brown color. At ordinary temperatures it is permanent. Acids even when very dilute decompose this body easily with evolution of hydric sulphid and separation of sulphur. Heated in a current of hydrogen it loses one atom of sulphur preserving its luster and crystalline structure. The new compound has the formula $K_2S.2FeS$, or,

$$\begin{array}{c} S-K \\ | \\ Fe^{IV}=S \\ | \\ S-K \end{array}$$

Schneider considers it probable that in fusing ferric oxyd with potassic or sodic carbonate similar compounds, containing oxygen in place of sulphur, are formed.

When bismuth is used in the above process in place of iron, delicate light steel gray brilliant crystalline needles are formed which have the formula $Bi_2K_2S_4$, or,

$$\begin{array}{c} S-K \\ | \\ S=Bi_2^{VI}=S \\ | \\ S-K \end{array}$$

This compound is easily and completely decomposed by chlorhydric acid with evolution of hydric sulphid. The author has obtained similar compounds containing copper, iron and copper, and platinum. They are beautifully crystalline and will form the subject of future more extended description.—*Pogg. Ann.*, B. 136, p. 460. W. G.

5. *On a new series of crystallized platinum compounds.*—By the action of stannous chlorid upon platinic chlorid, SCHNEIDER obtained a substance having probably the empirical formula, $Na_2Pt_2Sn_2O_6$, which though not yet thoroughly studied, forms the starting point from which the author obtained some very remarkable new compounds. When an intimate mixture of 4 parts of the above compound, 6 of potassic carbonate and 8 or 10 of sulphur are fused together, and the cold fused mass is treated with water, a cochineal red crystalline powder remains, easily obtained pure by washing. Under the microscope this compound is seen to consist of sharply defined six-sided tables of an almost metallic luster, which vary in color, according to their thickness, from light yellowish red to deep garnet red. The crystals are insoluble in water and unchanged by it. Dilute chlorhydric acid colors them at once dark greyish-black and takes up potash, but without evolution of any gas whatever. The residue is free from potash and is not attacked by boiling chlorhydric or nitric acid and but slowly by aqua regia. The original red body when heated in a current of hydrogen loses $\frac{2}{3}$ of its sulphur. The residue then gives up potash to chlorhydric acid, and the residue thus obtained loses all the remaining sulphur, when heated in hydrogen, and leaves a mixture of tin and platinum. From this it appears that the last $\frac{1}{3}$ of the sulphur is retained through the influence of the potash. The direct expression of the results of the analysis of the red compound is Sn O K Pt_3S_6. The simplest atomistic formula appears to be the following:

$$\begin{array}{ccc} Pt^{II}-S & & \\ | & & | \\ Sn^{IV}= & & S_2 \\ | & & | \\ K_2 & & O \\ | & & | \\ Pt^{IV}= & & S_2 \\ | & & | \\ Pt^{II}- & & S \end{array}$$

The action of chlorhydric acid on this salt is then readily explained, since, without writing the whole formula, we see at once that

$K_2\Theta + 2HCl = H_2\Theta + 2KCl$. The residue must therefore have the formula,

$$\begin{array}{l} Pt^{II} - S \\ | \\ Pt^{IV} = S_2 \\ | \\ Sn^{IV} = S_2 \\ | \\ Pt^{II} - S \end{array}$$

The action of hydrogen upon the new compound may then be expressed by the equation:

$$\begin{array}{l} Pt^{II} - S \\ | \\ Pt^{IV} = S_2 \\ | \quad | \\ K_2 \quad \Theta \\ | \quad | \\ Sn^{IV} \quad S_2 \\ | \quad | \\ Pt^{II} - S \end{array} + 8H = 4H_2S + \begin{array}{l} Pt \\ | \\ Pt^{II} - S \\ | \quad | \\ K_2 \quad \Theta \\ | \quad | \\ Sn^{II} - S \\ | \\ Pt \end{array}$$

Hence the residue consists of metallic platinum mixed with a simple oxy-sulpho salt in which tin and platinum are now diatomic. The action of chlorhydric acid on the oxy-sulpho salt may be represented by the equation:

$$\begin{array}{l} Pt^{II} - S \\ | \quad | \\ K_2 \quad \Theta \\ | \quad | \\ Sn^{II} - S \end{array} + 2HCl = H_2\Theta + 2KCl + \begin{array}{l} Pt - S \\ | \quad | \\ Sn - S \end{array}$$

The last term when heated in hydrogen then gives hydric sulphid and a mixture of tin and platinum. The corresponding sodium salt may be obtained in the same manner and closely resembles the potassic compound.—*Pogg. Ann.*, cxxxvi, p. 105. W. G.

7. *Contributions to a knowledge of conjugate bodies in inorganic Chemistry.*—Under this title Blomstrand has communicated a number of interesting notices of what may be termed atomic aggregation. Platino-cyanid of potassium takes up two atoms of iodine with great facility, forming an iodo-platinocyanid, $K_2Cy_4Pt + I_2 = K_2Cy_4PtI_2$, which crystallizes in large brilliant brown crystals. Chlorine and bromine displace the iodine, forming a corresponding chlorid and bromid, the former being the salt described by Knop. The corresponding salts of other positive metals are easily obtained. Iodine in like manner unites directly with auro-cyanid of potassium forming long brilliant brown needles, KCy_2AuI_2. Similar compounds of chlorine and bromine also exist. Double nitrite of platinous oxyd and platinum, $K_2Pt(N\Theta_2)_4$, also takes up chlorine and bromine readily, forming yellow crystalline salts which the author formulates as follows: $(K-\Theta-N\Theta=N\Theta-\Theta)_2PtCl_2$ and $(R-\Theta-N\Theta=N\Theta-\Theta)_2PtBr_2$.

Blomstrand regards the ammonia-nitrite of Lang $(NH_3)_2Pt.(N\Theta_2)_2$ as an intermediate term between the ammonia and the nitrous series and gives it the formula $(H_3N=N\Theta-\Theta)_2Pt$. This salt also unites with chlorine and bromine, forming very beautiful

crystals. The bromid has the formula $(H_3N{=}N\Theta{-}\Theta)_2PtBr_2$ and is the bromid of a true base in which the bromine may be replaced by oxygen acids. The author terms the base plato-nitrosamin. The paper contains many interesting and instructive illustrations of the author's peculiar views of the qualitative influence produced on compounds by the substitution of electro-positive or electro-negative elements—views which are set forth with much clearness and force in his recently published work "Die Chemie der Jetztzeit."—*Berichte der Deutschen Chemischen Gesellschaft*, 1869, No. 9, p. 202. W. G.

II. MINERALOGY AND GEOLOGY.

1. *On the Surface Geology of the Basin of the Great Lakes, and the Valley of the Mississippi;* by DR. J. S. NEWBERRY, (Ann. Lyc. Nat. Hist. N. York, ix, 213, 1869).—In this important paper the author, after reviewing the general facts with regard to the drift, remarks as follows on the history of the era:

"1st.—That in a period probably synchronous with the glacial epoch of Europe,—at least corresponding to it in the sequence of events,—the northern half of the continent of North America had a climate comparable with that of Greenland; so cold, that wherever there was a copious precipitation of moisture from oceanic evaporation, that moisture was congealed and formed glaciers which flowed by various routes toward the sea.

"2nd.—That the courses of these ancient glaciers corresponded in a general way with the present channels of drainage. The direction of the glacial furrows proves that one of these ice rivers flowed from Lake Huron, along a channel now filled with drift, and known to be at least 150 feet deep, into Lake Erie, which was then not a lake, but an excavated valley into which the streams of northern Ohio flowed, 100 feet or more below the present lake level. Following the line of the major axis of Lake Erie to near its eastern extremity, here turning northeast, this glacier passed through some channel on the Canadian side, now filled up, into Lake Ontario, and thence found its way to the sea either by the St. Lawrence or by the Mohawk and Hudson. Another glacier occupied the bed of Lake Michigan, having an outlet southward through a channel—now concealed by the heavy beds of drift which occupy the surface about the south end of the lake—passing near Bloomington, Ill., and by some route yet unknown reaching the trough of the Mississippi, which was then much deeper than at present.

"3rd.—At this period the continent must have been several hundred feet higher than now, as is proved by the deeply excavated channels of the Columbia, Golden Gate, Mississippi, Hudson, &c., which could never have been cut by the streams that now occupy them, unless flowing with greater rapidity and at a lower level than they now do."

After further explanations he takes up the subject of the *Drift Deposits.*

"The Drift deposits which cover the glacial surface, consisting of fine clays below, sands and gravel above, large transported boulders on the surface, and the series of lake ridges (beaches) over all, form a sequence of phenomena of which the history is easily read.

"The lower series of blue or red clays—the 'Erie clays' of Sir William Logan—over a very large area, rest directly on the planed and polished rock surfaces. These clays are often accurately stratified, were apparently deposited in deep and generally quiet water, and mark a period when the glacial ice-masses, melted by a change of climate, retreated northward, leaving large bodies of cold, fresh water about their southern margins, in which the mud produced by their grinding action on the paleozoic rocks of the Lake District was first suspended and then deposited.

"On the shores of Lake Erie these clays contain no boulders, and very few pebbles, while farther north and west boulders are more abundant. This is precisely what might be expected from the known action of glacial masses on the surfaces over which they pass. Their legitimate work is to grind to powder the rock on which they rest; an effect largely due to the sand which gathers under them, acting as emery on a lead wheel. The water flowing from beneath glaciers is always milky and turbid from this cause. Rocks and boulders are sometimes frozen into glaciers, and thus transported by them, but nearly all the boulders carried along by a glacier are such as have fallen from above; and a moraine can hardly be formed by a glacier except when there are cliffs and pinnacles along its course.

"In a nearly level country, composed of sedimentary rocks passed over by a glacier, we should have very little débris produced by it, except the mud flour which it grinds.

"The Erie clays would necessarily receive any gravel or stones which had been frozen into the ice, either as scattered pebbles or stones, distributed to some distance from the glacial mass by floating fragments of ice, or as masses of frozen gravel, or larger and more numerous boulders near the glacier. In some localities torrents would pour from the sides and from beneath the glacier, so that here coarse material would alone resist the rapid motion of the water, and the stratification of the sediments would be more or less confused."

The author next mentions the evidence of a general subsidence, greater to the north, and an ameliorated climate, as succeeding to the Glacial era; and then makes the following observations on the "*Yellow Sands and Surface Boulders.*"

"I have mentioned that on the Erie clays are beds of gravel, sand and clay, and over these again great numbers of transported boulders, often of large size and of northern and remote origin.

"These surface deposits have been frequently referred to as the direct and normal product of glacial action, the materials torn up and scraped off by the great ice ploughs in their long journeys from the North; in fact, as some sort of huge terminal and lateral moraines. I have, however, disproved, as I think, this theory

of their transportation in a paper published some years since (Notes on the Surface Geology of the Basin of the Great Lakes. Proc. Bost. Nat. Hist. Soc., 1863), in which it is urged that the continuous sheet of the Erie clays upon which they rest, and which forms an unbroken belt between them and their place of origin, precludes the idea that they have been transported by any ice-current or rush of water moving over the glacial surface: as either of these must have torn up and scattered the soft clays below.

"There is, indeed, no other conclusion deducible from the facts than that these sands, gravels, granite and greenstone boulders—masses of native copper, &c., which compose the superficial Drift deposits—have been *floated* to their resting places, and that the floating agent has been ice, in the form of *icebergs;* in short, that these materials have been transported and scattered over the bottom and along the south shore of our ancient inland sea just as similar materials are now being scattered over the banks and shores of Newfoundland.

"If we restore in imagination this inland sea, which we have proved once filled the basin of the lakes, gradually displacing the retreating glaciers, we are inevitably led to a time in the history of this region when the southern shore of this sea was formed by the highlands of Ohio, &c., the northern shore a wall of ice resting on the hills of crystalline and trappean rocks, about Lake Superior and Lake Huron.

"From this ice-wall masses must from time to time have been detached,—just as they are now detached from the Humboldt Glacier,—and floated off southward with the current, bearing in their grasp sand, gravel and boulders—whatever composed the beach from which they sailed. Five hundred miles south they grounded upon the southern shore; the highlands of now Western New York, Pennsylvania and Ohio, or the shallows of the prairie region of Indiana, Illinois and Iowa; there melting away and depositing their entire loads—as I have sometimes seen them, a thousand or more boulders on a few acres, resting on the Erie clays and looking in the distance like flocks of sheep—or dropping here and there a stone and floating on east or west, till wholly dissipated.

"These boulders include representatives of nearly all the rocks of the Lake Superior country, conspicuous among which are granites with rose-colored orthoclase, gray gneiss, and diorites, all characteristic of the Laurentian series; hornblendic rocks, massive or schistose, and dark greenish or bluish siliceous slates, probably from the Huronian; dolorites and masses of native copper apparently from the Keweenaw Point copper region.

"In the Drift gravels I have found pebbles and small boulders of nearly all the paleozoic rocks of the lake basin, containing their characteristic fossils, viz: the Calciferous Sandrock with *Maclurea*, Trenton and Hudson with *Ambonychia radiata*, *Cyrtolites ornatus*, Medina with *Pleurotomaria litorea*, Corniferous with

Conocardium trigonale, *Atrypa reticularis*, *Favosites polymorpha*, Hamilton with *Spirifer mucronatus*, &c.

"The granite boulders are often of large size, sometimes six feet and more in diameter, and generally rounded.

"Along the southern margin of the Drift area, especially on the slopes of the highlands of Northern Ohio, the Drift sands and gravels are of considerable thickness, forming hills of 100 feet. or more in height, generally stratified, but often without any visible arrangement. These deposits are very unevenly distributed, with a rolling surface frequently forming local basins, which hold the little lakelets or sphagnous marshes so characteristic of the region referred to. These are the beds to which I have alluded as constituting, in the opinion of some geologists, a great glacial moraine, but from the fact that they are locally stratified, and overlie the older blue clays, I have regarded them as transported, not by glaciers but by icebergs.

"Possibly some part of this Drift material may have accumulated along the margin of the great glacier, moved by its agency; but in that case we should expect to find in it abundant fragments of the rocks which outcrop in the region under consideration, whereas I have rarely, if ever, seen in these Drift gravels any representatives of the rocks underlying the South margin of the lake basin.

"By whatever agency transported, the Drift gravels have, like the boulders, for the most part come from some remote point at the North, and were once spread broadcast along the southern shore of the inland iceberg-bearing sea.

"In the retreat of the shore line during the contraction of the water surface down to its present area, every part of the slope of the southern shore, between the present water surface and the highest lake level of former times, *i. e.*, all within a vertical height of 300 feet or more, must in turn have been submitted to the action of the shore waves, rain and rivers, by which if, as is probable, the retrograde movement of the water line was slow, these loose materials would be rolled, ground, sorted, sifted and shifted, so that comparatively little would be left in its original bedding; the fine materials, clay and sand, would be washed out and carried farther and still farther into the lake basin, and spread over the bottom, to form, in short, the upper sand layers of the Drift."

Dr. Newberry closes with remarks on the origin of the great lakes, in which he states his conclusion that:

"1st. Lake Superior lies in a synclinal trough, and its mode of formation therefore hardly admits of question, though its sides are deeply scored with ice-marks, and its form and area may have been somewhat modified by this agent.

"2d. Lake Huron, Lake Michigan, Lake Erie, and Lake Ontario, are excavated basins wrought out of once continuous sheets of sedimentary strata, by a mechanical agent, and that ice or water, or both."

2. *On the Nature and Cause of the Glacial Climate*; by JOSEPH JOHN MURPHY, Esq., F.G.S. (Q. J. Geol. Soc., xxv, 350, 1869).—In the present paper I purpose to show how far I agree with, and where I differ from, Mr. Croll as to the views on the cause of the glacial climate set forth in his paper in the 'Philosophical Magazine' for August, 1864.

Mr. Croll's conclusions may be stated in the three following propositions:—

1. A glacial period occurs when the eccentricity of the earth's orbit is at a maximum, and the solstices fall when the earth is in perihelio and in aphelio.

2. Only one hemisphere, the northern or the southern, has a glacial climate at the same time.

3. The glaciated hemisphere is that of which the *winter* occurs in aphelio.

I agree with Mr. Croll as to the first two propositions, but differ as to the third. I believe that the glaciated hemisphere is that of which the *summer* occurs in aphelio.

The following propositions are self-evident when stated:—

When the eccentricity of the earth's orbit is very considerable, and the winter of either hemisphere occurs in perihelio and its summer in aphelio, the nearness of the sun in winter will cause a mild winter, and his remoteness in summer will cause a cool summer.

Conversely, in the opposite hemisphere at the same time, the winter will occur in aphelio and the summer in perihelio: the remoteness of the sun in winter will cause a cold winter, and his nearness in summer will cause a hot summer.

Suppose, for instance, that when the eccentricity of the earth's orbit is much greater than at present, the midwinter of the Northern hemisphere occurs in perihelio; then

the *Northern* hemisphere will have a *mild winter* and *cool summer*,
the *Southern* hemisphere will have a *cold winter* and *hot summer*.

So far (granting Mr. Croll's astronomical data, for which he cites Leverrier, and which I believe are indisputable) there is no room for doubt. I have now to discuss the question, what effect these diversities of climate will have in producing glaciation.

Mr. Croll thinks the hemisphere of *cold winter* will be the glaciated one. I think, on the contrary, the hemisphere of *cool summer* will be the glaciated one.

On this subject it is needless to attempt to make any deduction from theory. We have plenty of observed data; and I think I can show that they all go to prove a cool summer to be what most promotes glaciation, while a cold winter has, usually, no effect on it whatever.

Forbes, in his work on Norway and its Glaciers, p. 206, quotes "the excellent generalization of von Buch, that it is the temperature of the summer months which determines the plane of perpetual snow." This indeed is almost an identical proposition; for perpetual snow is snow that lies through the heats of summer; and

it would appear obvious enough, had it not been frequently overlooked, that it must be the temperature of summer which, other things being equal, determines the level of summer snow.

But, according to the same authority (Forbes's 'Norway and its Glaciers,' p. 206), "another cause affecting exceedingly the level of the snow-line is the amount of snow which falls."

These laws are illustrated in detail by the following table. In constructing it I have assumed, what is tolerably near the truth, that the temperature of the hottest month of the year decreases in ascending at the rate of 1° F. for every 300 feet. The temperatures are taken, as accurately as I have been able to do it, from Dove's map. My authorities for the heights of the snow-line are, for the first four, Durocher as quoted by Mr. Hopkins in the 'Proceedings of the Geological Society' for Dec. 17, 1851, for the rest, Mrs. Somerville's 'Physical Geography,' p. 314. The temperatures are in degrees of Fahrenheit. The heights are in feet.

	Temperature of hottest month at sea level.	Height of 32° F. in hottest month.	Height of snow-line.
Pyrenees	74·5	12750	9300
Caucasus	77	13500	10300
Mont Blanc	72·5	12150	9000
Bernese Alps	72·5	12150	8800
Scandinavian Fjelde, 61° 43′ N	59	8100	5500
Mageroe, Norway, extreme north	45·5	4050	2160
Himalaya, about 31° N., *north side*	83·75	15525	16620
The same, " *south side*	83·75	15525	12980
Andes, near Quito	79·25	14175	15795
" 18° N.	81·5	14850	14772
" near Valparaiso	68	10800	12780
" 37° 40′ S.	63·5	9450	7960
Straits of Magellan	45·5	4050	3390

It is evident by this table that the snow-line rises above the line of 32° for the hottest month of the year where the snow-fall is small, and sinks below it where the snow-fall is great. In the Caucasus, the Alps, and the Pyrenees, the snow-line is about three-fourths of the height of the line of 32° for the hottest month of the year; in the Fjelde of Norway, about two-thirds; in the Peruvian and Chilian Andes above, but in Patagonia and Tierra del Fuego below; above, on the north side of the Himalaya, but below on the south side. These contrasts are all to be explained by the difference in the amount of snow-fall, which is greater on the south than on the north side of the Himalaya, greater in Patagonia and Tierra del Fuego than in Chilé and Peru, and probably greater, at least in winter, in Norway than in Central, Southern, or Eastern Europe.

The dependence of the height of the snow-line on summer temperature and on amount of snow-fall, to the exclusion of winter temperature, may be best shown, perhaps, by two extreme cases. The mean temperature of the Altai mountains (according to Mr. Hopkin's paper cited above) is below freezing; yet in consequence of the comparatively warm summer, and the small snow-fall, the

height of the snow-line (Mrs. Somerville's 'Physical Geography,' p. 61) is about 6000 feet. On the Straits of Magellan, on the contrary, though the mean temperature is several degrees above freezing, the height of the snow-line (see table) is little more than half as much.

It is well known that, other things being equal, the magnitude of glaciers depends on that of the snow-fields in which they rise; and as of course any depression of the snow-line will enlarge the snow-field, it follows that the lower the snow-line the further will the glaciers descend below it. As a decrease of about 3° F. is due to every 1000 feet of ascent in the hottest month, it follows that a fall of temperature to that extent in the hottest month would lower the snow-line by about 1000 feet; and in many cases it is likely that the glaciers in such a case would descend 1000 feet further below the snow-line than at present, thus gaining a total increase of 2000 feet of descent. This might not have much effect on the climate of Central Europe, but it would have a very great effect in those high latitudes where the glaciers would reach the sea and give origin to icebergs; for we know that icebergs have great influence as transporters of cold.

In particular cases the effect of a comparatively slight fall of summer temperature would be very great. I quote from Forbes' 'Norway and its Glaciers,' p. 215:—

"Though the surface actually covered by perpetual snow in Norway be small, yet the mountainous districts and tablelands everywhere approach it so nearly that the snow-plane may be said to *hover* over the peninsula, and any cause which should lower it even a little would plunge a great part of the country under a mantle of frost."

And again, p. 243:—

"It is exceedingly probable that a diminution of the temperature of the summer months by 4° only would at once place *one-fourth* of the surface of Norway within the snow-line; and so vast a mass of snow would refrigerate the climate, especially the summer temperature, to such a degree as would unquestionably pour glaciers into the head of every fiord in western Norway. . . . The lowering of the snow-line over so large a surface would deteriorate the climate and lower the mean temperature, which would lower the snow-line still further."

The change in the eccentricity of the earth's orbit is in all probability amply sufficient to account for this or a much greater change in summer temperature.

I take the following data from Mr. Croll's paper. The recently ascertained error in the old determinations of the sun's distance affects both distances alike, and consequently does not affect their ratio. Along with the maximum distances of the sun at present and at greatest eccentricity, I state the proportionate quantities of heat the earth will receive under those two different conditions:—

	Sun's maximum distance.		Ratio of heat received.
At present	96,473,205	miles	100
At greatest eccentricity..	102,256,873	"	90

So that in the one case the earth receives about one-tenth less heat than in the other.

The sun's maximum distance occurs at present a little after the midsummer of the northern hemisphere. When it occurred at the same time of the year during the period of greatest eccentricity, the earth at our midsummer was receiving only nine-tenths of the quantity of heat which it now receives at that time of the year. I cannot calculate the effect on climate; but it must have been very great, not only directly, by depressing the snow-line, but as Forbes remarks in the place cited above, indirectly by chilling the air—and I will add, by filling the North Sea with the icebergs which must have broken off from the glaciers that filled the Norwegian fiords, as they do now from the glaciers of Greenland. We have plenty of evidence of iceberg action during the glacial period.

I believe I have shown that glaciation depends chiefly on a cold summer, but partly also on an abundant snow-fall. I have now to show that a period of cold summers, caused as I have explained, must be also one of snowy winters; so that the two conditions favorable to glaciation will occur together.

During the mild winter of the glaciated hemisphere, there is a hot summer in the opposite one. Increase of temperature promotes increase of evaporation in a much greater ratio than that of the increase of temperature; and increased evaporation in the summer hemisphere will produce increased snow-fall in the winter one. We know that at present the vapor raised in one hemisphere is to a great extent precipitated in the other; for, were it not so, the southern hemisphere, by reason of its greater extent of ocean surface, would have a rainier climate than the northern; and such does not appear to be the case on the whole. Besides, during a glacial period, the atmospheric circulation between the two hemispheres, at the time of the earth's minimum distance from the sun (which on my theory was in the winter of the glaciated hemisphere), must be more active than ever it is now; for when the earth, at either solstice, was nearer the sun than is ever the case now, and the difference of temperature between the two hemispheres consequently at its greatest possible amount, this would produce a very active circulation of atmospheric currents between the two hemispheres, which would involve the deposition as rain or snow in the winter hemisphere of a great part of the moisture evaporated in the summer one.

[The author continues with remarks on fiords as results of the Glacial era, a subject long since discussed in this Journal.]

3. *Geological Report of the exploration of the Yellowstone and Missouri Rivers;* by Dr. F. V. Hayden, Assistant under the direction of Captain (now Lieut. Col. and Brevet Brig. Gen.) W. F. Reynolds, Corps of Engineers, 1859–60, with a colored map. 174 pp. 8vo. 1869.—Several years have elapsed since the explorations for this report were made, and many of its general conclusions have appeared, through the author, in this Journal. The details here presented will be read with much interest. The

map is large and very instructive, showing, more accurately than had been done before, the distribution of the Tertiary and Cretaceous formations, and the girt of Carboniferous and Potsdam rocks around the high metamorphic ridges of the summit. Some further explorations of the mountains are required to make certain all the points in this distribution.

The last thirty pages of the volume are occupied by a report on the Cretaceous and Tertiary plants of the region, by Dr. J. S. Newberry, now Professor of Geology and Paleontology in the School of Mines, Columbia College, New York. Dr. Newberry's extensive knowledge of the fossil Botany of North America has enabled him to give his memoir great value. Dr. Hayden's absence in the mountains, during the past year, while his report was in press, accounts for its many typographical errors.

In the exploration of last year here referred to, Dr. Hayden was engaged in a new Government Survey under the direction of the Department of the Interior; and the first pages of the "Preliminary Field Report of the Survey" have been received by us. The Report promises to be one of great value to the science, and a notice of its results may be looked for in our next number. The Hon. T. D. Cox, Secretary of the Interior, states in his annual Report for the year 1869, that "Dr. Hayden entered on his labors in the field the last of June, at Cheyenne, Wyoming Territory. His route was through Denver, the silver and gold mining region of Georgetown and Central City, the Middle Park, Colorado City, and Fort Union to Santa Fé, returning through the San Luis Valley and South Park to Denver. The exploration, though brief and rapid, was eminently successful, and the collections in geology, mineralogy, botany, and zoology were extensive. His preliminary report bears date Oct. 15. It is accompanied by two other reports made to him by his assistants—one on mines and mining, the other on agriculture. These papers are a valuable contribution to our knowledge of the subjects which they embrace, and merit careful perusal."

4. *Mineralogy Illustrated;* by Dr. J. G. v. Kurr, Prof. Roy. Polytechn. School at Stuttgart, etc. 22 large colored plates interleaved with explanatory text.—The figures in this Atlas represent in colors crystals or massive specimens of various minerals, especially the ornamental species, and is intended to aid the student in their identification. The figures are in general well colored; and although the text is not wholly free from its German idiom, the work may be of value in the study of the science.

5. *Tableau Mineralogique;* by M. Adam, Commander de la Légion d'Honneur, etc. 102 pp. 4to. Paris, 1869.—A classified catalogue of the minerals in the splendid collection of Mr. Adam, of Paris, with a brief statement of the character and composition of the species.

III. BOTANY AND ZOOLOGY.

1. *Botanical Notabilia.*—Some announcements of recent publications with cursory remarks, and items of intelligence are here brought together in the expectation that they may be interesting or useful to botanical readers of the Journal. The only one of the publications here mentioned which can be said to possess much general interest is the first, viz:

An *Address of George Bentham, Esq., President of the Linnæan Society, London*, read at the Anniversary Meeting, May 24, 1869. It is the latest one of that series of pertinent annual discourses by which Mr. Bentham's presidency has been distinguished, and which it is hoped he will continue. A preceding one was reprinted in this Journal, and it would be well that this should be also. The topic is *Geographical Biology*, considered first for plants and then more succinctly for animals. Although distribution is one of the strongest points of derivative doctrine, yet it is wonderful to see, in the light of this sober and impartial survey, how entirely the whole aspect of philosophical natural history in this regard has changed within two decades. "Centres of creation," and the like are of the language of the past, here replaced by Bentham's happy term of "areas of preservation." And the conclusion, tardily reached "that the present geographical distribution of plants was in most instances a derivative one, altered from a very different former distribution," has been followed by the conviction that the present species themselves are equally derivative, and have a changeful history, some steps in which may be dimly surmised by the study of cognate forms, extant or fossil. At the point now reached, if not by general yet by large consent, the problems we are led to consider are such, that it is indispensable to have a term of wider application than "species" technically means; and Mr. Bentham here appropriates to this use the word *Race*, to denote either permanent variety (the old meaning of the word when definitely restricted), or species, or groups of two or more near and so-called representative species, i. e., for those collections of individuals or resembling groups of individuals whose association in the way of lineage is taken for granted by this class—or rather by these classes—of naturalists. As the term was only beginning to get fixity in its restricted sense, it will take the wider sense without confusion or difficulty, and with the advantage of a vernacular instead of a new-coined purely technical word.

The Miscellaneous Botanical Works of Robert Brown, which Mr. Bennett has edited for the Ray Society (2 vols. 8vo), are now completed by the Atlas of Plates, in small folio, 38 plates. The best thanks of Botanists, especially of the younger ones, are due to Mr. Bennett and to the Council of the Ray Society.

Another publication by the Ray Society for which botanists will be grateful is the *Vegetable Teratology, an Account of the principal Derivations from the usual construction of Plants*, by Dr. Masters. It was published last autumn, and forms an 8vo volume of nearly 600 pages, illustrated by over 200 wood-cuts.

The collection of cases is ample, the arrangement clear and scientific, and the general handling of morphological questions and theories of structure shows a full acquaintance with the subject. One cannot here well advert to points which invite discussion or further remark. Indeed a treatise of this character and upon such topics ought to be reviewed *in extenso*. It should be observed in passing that it may not be quite correct to speak of DeCandolle as "giving in his adhesion to the morphological hypothesis of Goethe (p. xxii, also p. 476): it is generally understood that when the *Théorie Elémentaire* and the *Organographie* were written DeCandolle knew nothing of Goethe's essay, and had never heard of Wolff.

A memoir of the Life of the late Prof. Wm. H. Harvey, principally made up of his own inimitable letters, edited by a surviving relative, has lately been published by Bell and Dalby, London. The book was at first intended for private distribution; but it was well decided, as his many friends and admirers in this country will be glad to know, that it should be issued through a publisher in the usual way. It makes an octavo volume of goodly size. Such a charming photograph of a lovely character will interest many to whom Dr. Harvey was personally unknown.

The *Genera of South African Plants* was one of the late Dr. Harvey's first undertakings when established, it was thought for some years, at the Cape of Good Hope. It was published at Cape Town, in 1838, and most useful it was in inspiring and developing the study of Botany in the Colony: nor is its usefulness superseded by the elaborate *Flora Capensis*, which was carried on with remarkable promptitude while Dr. Harvey lived, but which remains unfinished. He had prepared in a good degree the materials for a new edition of the *Genera*, which has now been edited by Dr. Hooker, the succinct introduction to Botany originally prepared by Mr. Bentham for his British Flora, and since added to all the Colonial Floras, being prefixed.

As to these Colonial Floras, that of Australia is pushed forward with Mr. Bentham's customary vigor, under the great advantage of his unsurpassed knowledge and his enviable opportunity of being able to devote his whole time without distraction to Systematic Botany. The fourth volume of the *Flora Australiensis* appeared almost a year ago: it contains most of the Monopetalous orders after the *Compositæ*, excepting *Verbenaceæ*, *Labiatæ*, &c., which are probably already in type for the fifth volume. Under *Breweria* Mr. Bentham justly remarks that this genus ought not to have been referred to *Bonamia* of Madagascar, as I had rashly done; yet he is wrong in the supposition that Thouars' plant not Convolvulaceous, but allied to *Ehretia* and *Cordia*. On inspection it proves to differ from *Breweria*, and to agree with *Cressa*, in the corolla, which not plaited in æstivation. There are probably two species.

The *Flora of Tropical Africa* by Prof. Oliver, "assisted by other botanists," though not exactly a Colonial Flora, is upon the same model. It is founded upon the African collections which have accumulated at Kew. Considerable as they are already, they

represent the vegetation only of the outskirts of a vast *terra incognita*, of which we may hope to know something ere long, and which may give to this work as it advances an exceeding interest. This first volume, which was published in 1868, includes the orders antecedent to *Leguminosæ*. The *Malvaceæ* and their allies are by Dr. Masters, who has paid much attention to the structure of the flowers of this group; the *Ampelideæ*, *Sapindaceæ* and *Connaraceæ* are by Mr. Baker.

Dr. Oliver has just published a neat little *First Book of Indian Botany*, on the basis of his well-known elementary *Lessons* for home use, only the illustrations of the natural orders, the examples, &c., are from Indian plants, and considered in view of Indian Botany.

The ninth part of Dr. Seemann's *Flora Vitiensis*, nearly concluding the *Phanerogamia*, has been for some time issued; and the 10th, which will complete this laborious work, is in press. The Ferns are by Dr. Caruthers of the British Museum, so favorably known for his researches in fossil botany.

Mr. Baker, the Assistant Curater of the Kew Herbarium, after completing the *Synopsis Filicum* which was barely commenced by Sir Wm. Hooker, has made the results of that work the basis of a very interesting paper *On the Geographical Distribution of Ferns*, in the last (26th) volume of the Transactions of the Linnæan Society.

The publication of Sir Wm. Hooker's *Icones Plantarum* closed a dozen years ago with the 10th volume, completing a thousand plates. Dr. Hooker with the assistance of Mr. Bentham and Prof. Oliver, has now commenced a third series, of which two parts are issued, one in November, 1867, the other in June, 1868, and a third part is in press. The plates are numbered on from volume 10 (which is a convenience in citation) viz: 1001 to 1050. They are taken from plants in the Kew Herbarium, and will serve to illustrate some of the work going on in that richest of botanical collections, and notably, as it proceeds, the *Genera Plantarum*. Thus far nearly all are from the southern hemisphere. Two plants, however, are North American, viz: *Arceuthobium brachypodum* of Engelmann, from New Mexico, in which Prof. Oliver directs attention to some apparent peculiarity of the ovule, which needs investigation in the fresh plant; and *Leitneria Floridana* of Chapman, which Prof. Oliver describes as having a thin but evident layer of albumen around the embryo, and he agrees with Chapman in referring the genus to *Myricaceæ*, although with misgivings.

A whole volume of the *Journal of the Linnæan Society* (the twelfth, issued in advance of the eleventh), is occupied with an enumeration and description of all known Mosses of South America, by Mr. Mitten, founded primarily on Spruce's collections, which have been distributed into sets and disposed of. Mitten's *Musci Austro-Americani* thus forms a substantial 8vo volume of 659 pages. A double number of the eleventh volume is equally devoted to Mr. Spruce's South American collections and researches, viz., to the *Palmæ Amazonicæ*, an account by himself of the

Palms of the regions visited by him, a paper rich in generally interesting and readable as well as technical scientific matter.

Dr. Anderson has a paper on the Palms of Sikkin in the preceding number of the same volume; Dr. Kirk writes on the Copal of Zanzibar, and makes out that the old or fossil copal is the produce of the same tree, a species of *Trachylobium* from which recent Zanzibar copal is at present yielded. The other articles most noteworthy for us in the eleventh and the later numbers of the tenth volumes, are one by A. W. Bennett of London on *the Structure and Affinities of Parnassia*, upon which there is somewhat to be said whenever the present writer has opportunity to make re-examinations in the view of correcting certain probable mistakes or oversight. *On chemical reaction as a specific character in Lichens*, by Dr. Lauder Lindsay, who comes to the conclusion that these chemical tests introduced of late are of little value or reliability. *Notes on Jussiæa* by Charles Wright, a letter to Dr. Hooker, showing what excellent characters, hitherto nearly overlooked or in some cases misapprehended, are furnished by the seeds, &c., in this genus: and finally *Fungi Cubenses*, by Messrs. Berkeley and Curtis, founded on the very ample and largely novel collections in this order made by Mr. Charles Wright. It is to be stated here that these Fungi, accurately determined by the above distingnished mycologists, have now been distributed into sets, and placed on sale. Application for the few remaining sets may be made by letter to Mr. Wright, at the Harvard University Herbarium, Cambridge, Mass.

General Munro, who, when stationed in Canada, did much for American Agrostology, and now in England is able to command time for systematic work in his favorite order, has published in the Transactions of the Linnæan Society, a Monograph of *Bambusaceæ*. These, as now known, amount to upward of 170 species, belonging to 20 genera, our Cane of the Southern States being the solitary representative of the tribe in this country. The rare flowering which renders the study of the group difficult, is a common, though not universal feature. The common Bamboo of India is thought to blossom in any district only once in thirty or forty years, and when a general flowering occurs, the copious seed, giving sustenance to thousands, has often averted a famine.

For a French edition of his little work on the Fertilization of Orchids by insects, Mr. Darwin had occasion to prepare an appendix, enumerating the principal contributions to our knowledge of this subject, which have been made since this volume called attention to it and made the enquiry popular. There being as yet no call for a new edition in England, this appendix, of a few pages in extent, was turned into English and printed last summer in the Annals and Magazine of Natural History.

Dr. Baillon, now the Professor of Botany in the School of Medicine, Paris, commenced the publication of the "*Adansonia, Receuil Periodique d' Observations Botanique*" (8vo,) in 1860, and the 8th volume was completed in the summer of 1868. Each volume is illustrated by ten or twelve plates.

A large portion of the articles are by the able and indefatigable editor, the first being his essay on the female flower of *Coniferæ*, and against gymnospermy, which at the time attracted much attention. (Apropos to which, it is said that the doctrine of *gymnospermia* was first propounded by Targioni Tozetti. I have not seen the passage in which it is said to be recorded.) A second article describes, earlier than elsewhere, the setiform bodies which stand in place of petals in *Asarum Europæum*; but states that they are wanting or generally so in *A. Canadense;* they are, however, very commonly present. In the seventh and eighth volumes are brought together a very interesting collection of Trécul's various papers on the latex, in which he shows that this juice is contained in the spiral, dotted, and other ducts as well as in the so-called vessels of the latex.

Prof. Baillon commenced in 1868, a work on Organogeny, like that of Payer's, of which it would be a sort of supplement, but extending to the fruit as well as the flower. Only a single fasciculus has yet appeared of this *Traité du Developement de la Fleur et du Fruit*, (V. Masson et fils), with one plate, devoted to the flower of *Santalaceæ*. A more generally interesting work of the same author, and one which is pushed forward with vigor, is his *Histoire des Plantes* (imperial 8vo. Hachette & Cie, Paris), in a series of monographs of families, with wood-cut illustrations, the text with full readable details in French, with copious references, and then full generic characters in Latin. The three parts already published (1868–1869), comprise the *Ranunculaceæ*, the *Rosaceæ*, the *Connaraceæ* and the *Leguminosæ-Mimoseæ*. To give an idea of Baillon's valuation of genera, and how greatly he would reduce them, it may be noted that he refers back both *Eranthis* and *Coptis* to *Helleborus*, joins *Caltha* as well as *Calathodes* to *Trollius*, *Aconitum* to *Delphinium*, *Trautvetteria* to *Ranunculus*, and *Cimicifuga*, to *Actæa;* moreover, he refers the latter genus along with *Thalictrum* to the *Clematideæ*. *Crossosoma* he doubtfully places by the side of *Pæonia*, which appears to be the best that can be done with it, giving weight rather to the perigynous petals and stamens than to the arillus. He calls attention to his discovery, first recorded in the Adansonia, of three or four additional but minute and early abortive ovules in *Anemone* and *Clematis*. In *Rosaceæ*, beyond the reductions made by Bentham and Hooker, *Chamærhodos* also is united with *Potentilla*, and *Waldsteinia* as well as *Coluria* with *Geum;* Torrey's *Coleogyne* is well placed next to *Cercocarpus* and *Purshia;* but Baillon is much mistaken in supposing that he was the first to refer *Lutkea* or *Eriogynia* to *Spiræa*; it was so referred in the Flora of N. America more than a quarter of a century ago. In Torrey's *Emplectocladus* Baillon has rightly detected a *Prunus*, not far removed from Humboldt's *Amygdalus microphylla*.

It is rather late to notice Burean's *Monographie des Bignoniacees* (Baillière, 4to) of which the two parts issued bear the date of 1864, but the work is hardly known in the country and has not advanced far. The 200 or thereabouts pages of letter-press are given to a

prolix account of the history of the classification of the order, its affinities, organography, &c. The thirty-one plates appear to be excellent. There is a figure of our *Bignonia capreolata* L., under the name of *Anisostichus capreolata*, of *Tecoma stans* as *Stenolobium stans* Seem., of *T. radicans* as a *Campsis* (in the letter-press), and of *Catalpa bignonioides*.

The other section of the 16th volume of *De Candolle's Prodromus* has just been issued. The two parts form indeed independent volumes, and are paged and indexed as such, so that for all time botanists will have to quote *D C.* Prodr. 16 (1), p., &c., which is to be regretted, but there is no help for it. The present (prior) part, of 450 pages besides 65 intercalated ones, contains the *Buxaceæ* and some other plants excluded from the *Euphorbiaceæ*, by Dr. Müller; the *Empetraceæ* by Alph. DeCandolle himself (*Empetrum* reduced to one species, *Corema* of two, and a *Ceratiola*); *Cannabineæ* by the same (the *Ulmaceæ* and *Artocarpeæ* postponed not being ready), the *Urticaceæ* (i. e. the *Urticeæ*) by Weddell; *Piperaceæ* by Casimir DeCandolle (the *Saurureæ* made a mere tribe, and the *Pipereæ* mainly included under Piper of 635 species, *Peperomia* of 389!); *Chloranthaceæ* by Solms-Laubach of Halle, reduced to three genera; and finally *Garryaceæ* by the editor, comprising nine species of *Garrya*. It appears that the latter end of the volume was printed first, which explains the omission of *G. buxifolia*, a species discovered in Northern California by Bolander, and published a year and a half ago. If one ort wo collaborators will now bring up their arrears, the editor may very soon have the great satisfaction of announcing the completion of the great Dicotyledonous series.

The incomparable *Flora Danica* goes on, and the 47th part, with plates 2701 to 2760, has come to hand. Among the plants of interest to us which it contains is *Pyrus* (*Sorbus*) *Americana*, our Mountain Ash, with the red petioles and pointed leaflets, from Greenland.

In the Journal of the Natural History Society of Copenhagen (Vidensk. Middel. Naturhist. Forening Kjobenhavn) for the year 1866, issued in 1867–8, Oersted has an elaborate paper on the classification of Oaks, with a catalogue of all the species, and many illustrations, both copper plates (of female flowers) and woodcuts; of the latter, the cuts from impressions of the leaves, showing the venation, are peculiarly excellent. Being a later version of *Quercus* than that of DeCandolle, and with points of its own, it is commended to the attention of Dr. Engelmann, who is likely to have most to do with the American species. To illustrate venation and the nature of the surface of foliage, photography may be turned to good account, far more than is now commonly thought of. We have seen a photograph from a specimen of one of the coriaceous-leaved Oaks of the Dutch Indies which was truly wonderful in its rendering.

Dr. Lange, in the same publication for 1867 (issued in 1868), describes the *Pyroleæ* and *Monotropeæ* of Mexico and Central America. Four of the former group are identified with United

States species; and the fifth, *Pyrola Liebmanni* of Lange may be added, as it is apparently undistinguishable from one of our Western forms which already has several names, among them *P. bracteata*. Zuccarini's *Monotropa coccinea* is described in full and beautifully figured. The lively red color, in which it chiefly differs from *M. uniflora*, may not be of specific importance, considering that what we take for *M. Hypopitys* in the United States is often flesh-color or red in the warmer parts of the country.

A far more interesting Monotropeous plant has been collected by the French expedition to Mexico, viz: *Pleuricospora* Gray, (in Proceed. Am. Acad. 1867), with mature fruit, well showing the parietal placentation, and the close-coated seeds.

Prof. Bunge of Dorpat has brought out the first part of his Monograph of the *Astragali* of the Old World. It forms a fasciculus of 140 pages, in the current volume (11 of the 7th series) of the Memoirs of the St. Petersburg Academy of Sciences (1868), and contains the *Claves Diagnosticæ* of all the species, under their subgenera and sections. We have a goodly number of species in this country, mainly beyond the Mississippi; there are about 200 known in America,—a small matter compared with those of the Old World, of which Bunge here characterizes a little short of a thousand species! It is a subject of congratulation that, thanks to the author's great carefulr ess and knowledge there is hardly any double employment of specific names for *gerontogæan* and *neogæan* species. It would have been better to have avoided the use of names for sections in the same form as those of genera by adopting a plural termination or some like device.

A remarkable parasitic plant of the Sonora desert, indicated in 1854 in *Plantæ Thurberianæ*, but fully published and illustrated as much as ten years later by Dr. Torrey in the *Annals of the New York Lyceum*, his *Ammobroma Sonoræ*, has recently been investigated, along with its allies, by Count Solms-Laubach, a young botanist of high promise, now resident at Halle. It was thought that the incomplete specimen brought by Col. Gray and now belonging to Dr. Torrey was the only one extant. But it appears that much better ones were gathered by C. Schukard, draughtsman of the party, and sent along with a drawing, to Dr. Behr of San Francisco, who presented them to the late Prof. Schlechtendal of Halle. Solms-Laubach, a former pupil of Schlechtendal, found the specimens in his herbarium, also good ones of *Lennoa madreporoides*, and having likewise examined Kunth's *Corallophyllum cœruleum*, had thus before him all the known representatives of the group except Nuttall's *Pholisma*, which is known in Europe only by the specimen in the Hookerian Herbarium which served as the original of the figure in the *Icones Plantarum*, and probably by one in Nuttall's own herbarium now belonging to the British Museum. The results are just published in an elaborate memoir: "*Die Familie der Lennoaceen, von Hermann Grafen zu Solms-Laubach*," a separate issue from the eleventh volume of the Transactions of the Natural History Society of Halle, bearing the date of 1870. The three plates illustrate in detail the three plants

examined, viz: *Ammobroma Sonoræ* Torr., *Lennoa madreporoides* Llav. & Lex., and *L. cærulea* Solms, which is Kunth's *Corallophyllum cæruleum.* The order *Lennoaceæ* of Torrey is divided into two sections, 1st, with stamens in one rank and the anther-cells parallel for *Pholisma* and *Ammobroma*, each of a single species; 2nd with the alternate stamens inserted lower than the rest, and the anther-cells contiguous at the apex but diverging below, for *Lennoa*, of two species. Solms doubts, and with good reason, whether *Ammobroma* should not be referred to *Pholisma*, which would leave two very well defined genera. The difference in the ovule, whether horizontal or perpendicular becomes of small consequence when it is seen that the rhaphe in the former case is on the lower side, indicating a pendulous ovule. By several pertinent considerations Solms reinforces Dr. Torrey's opinion that the affinities of these plants are with the Erical alliance and not at all with the *Orobancheæ;* but the insertion of the stamens, even more than the structure of the ovary and fruit, forbids union with the *Monotropeæ.* That character belongs, however, to a great part of *Epacrideæ*, and Solms shows us that the structure of the ovary is the same as in some of the latter, and in certain *Vaccineæ.* The cells, as he shows, are half carpels, each carpel being biovulate and divided into two uniovulate *locelli* by intrusion from the back, as in *Gaylussacia.*

Professor Braun, continuing his study of the *Rhizocarpeæ* and the like, published last year a little monograph of the Australian species of *Isoetes*, with a preface containing general remarks on those of Europe and North America. He has also published more extensively upon *Characeæ;* and Dr. Leonhardi, the Professor of Philosophy in the University of Prague has discussed the European species in detail in the Proceedings of the Natural History Society of Brunn, and in the *Lotos.* We have from Dr. Hegelmaier a second dissertation on *Callitriche*, aided as to the American species by Dr. Engelmann, who placed the notes and specimens of his herbarium at his disposal. He recognizes *C. marginata* Torr., in Bolander's collection; *C. microcarpa*, a new species of Engelmann from Wright's Cuban collection; *C. Japonica* of the same, collected by Wright in Japan; and describes a new California species, *C. Bolanderi.* Hegelmaier has published his Monograph of *Lemnaceæ*, in 4to, but we have not yet seen it.

Prof. Miquel has carried on the *Annales* of the Leyden herbarium as far as to the fifth fascicle of the fourth volume; the papers in the current volume thus far relate almost wholly to the botany of the Indian Archipelago.

The seventh volume of Walpers' *Annales Bot. Systematicæ*, continued by Dr. C. Müller, gets on but slowly: the first and second fascicles bear the date of 1868, and we have only the third for 1869, which comes down only to the *Zygophylleæ.*

Dr. Pritzel's *Iconum Botanicarum Index Locupletissimus*, a perfect model for typography and arrangement as well as careful editing, was supplemented in 1866 by a *pars altera*, of nearly 300 pages, bringing down the references to plates to the end of the

year 1865. The first edition of the main work has been reprinted verbatim, along with this supplement, instead of redigesting the whole, which would have been preferable. But it will temporarily do very well as it is, if the editor will soon re-edit the whole, incorporating into one alphabetical order the plates published down to 1870.

In the summer of 1868 appeared the 44th, 45th and 46th fascicles of Martius' *Flora Brasiliensis*, comprising the *Loranthaceæ*, a masterly work by Dr. Eichler; the *Oleaceæ* and *Jasmineæ* by the same (the character of *Chionantheæ* "semina exalbuminosa" still kept up, though it is not so either in *Chionanthus Virginica* itself or in the original *Linociera ligustrina;* see Proceed. Amer. Acad., 5, p. 331, etc.); the *Loganiaceæ* by Progel, and the *Styraceæ* by Seubert. Since the lamented death of Von Martius, Dr. Eichler has brought out the *Balanophoreæ* (in fasc. 47), with an admirable investigation and illustration of the intimate structure or anatomy as well as of the floral characters and morphology of this family, and has clearly shown it to have the ovulation of *Santalaceæ* and *Loranthaceæ* (some of them very exactly of the former), and to belong to that alliance.

The 48th part, the *Convolvulaceæ* by Meissner, has just been issued, but has not yet come to hand. Before his death, Von Martius had fortunately completed such arrangements with the Brazilian Government on the one hand and with Dr. Eichler on the other, as to secure the continuance and completion of this great work, that is, if it can be done within ten years; and Dr. Eichler is pushing it forward vigorously. He is placed at disadvantage, however, by the seemingly ill-advised refusal of the Bavarian Government to purchase and retain in Munich the library and herbarium of the *savant* who has done far more than any one else for natural science in Bavaria; and so the library of Martius is to be dispersed by auction next March; and the herbarium, which his will forbids the dispersion of, still awaits a purchaser. A pamphlet, *Das Herbarium Martii*, gives particulars of the contents of this collection. It comprises, 1, the general herbarium, estimated to contain about 60,000 species in 300,000 specimens, over half the species South American, especially Brazilian; 2, The Palm collection, which ought to be of very great importance; 3, A collection of fruits and seeds; 4, of woods; 5, a very rich and well-prepared collection of drugs and economical products, the larger part of which was formed by his brother Theodore Martius, when Professor of Pharmacy at Erlangen. Here is an unusual opportunity for some American University.

We have received a second edition (1864) of an *Atlas des Pflanzengeographie*, by L. Rudolph of Berlin, (published by Nicolas),* intended for schools, &c. It is well done, and if in English would answer an excellent purpose in our higher schools, or wherever Botany and Physical Geography are taught. It is strictly an Atlas, of nine folio plates or maps, charged with details, which

* Received from E. Steiger, German bookseller, Frankfort St. New York, along with a copy of Pritzel's Index (mentioned on the preceding page.)

are printed partly in colors, and with descriptive letter-press on the reverse. The centennial anniversary of the founder of Plant-geography should awake new interest, and give increased popularity to the subject.

The new President of the ancient Imperial Academy Naturæ Curiosorum, succeeding Carus, is Professor Behn of Hamburgh.

An interesting bit of botanical literature, is Pursh's *Journal of a Botanical Excursion in the Northeastern parts of Pennsylvania and New York, during the year* 1807. Dr. Thomas P. James found the manuscript journal of Pursh with some other papers accompanying the herbarium of Dr. B. S. Barton, in the possession of the American Philosophical Society, and had it printed in successive numbers of Meehan's *Gardener's Monthly*, from which it is now collected in the form of a little volume of eighty-seven pages. It will be remembered that this journal contains Pursh's notes made at the time of his discovery of *Scolopendrium*, and the station is indicated with sufficient precision to be determined, as was done, we believe, by Mr. Paine. If the Fern no longer exists at that very station, we know of it at three or four neighboring localities. Pursh's quaint and simple narrative of his wanderings and hardships is well worth perusal.

The Botanical Necrology for the year 1869, may happily be dismissed in few words.

Antonio Bertoloni, the most venerable botanist of our day, who has occupied the chair of botany in the ancient university of Bologna for considerably more than half a century, died, in the full possession of his mental powers, on the 17th of April, shortly after the completion of his ninety-fourth year. The very next day died the equally distinguished professor of the sister University of Turin, *Giuseppe Moris*, in the seventy-third year of his age. At Prague on the 28th of July, died the professor of Physiology, *J. E. Purkinje*, at the age of eighty-two. He was known in botany as the author of a neat little treatise, "*De cellulis antherarum fibrosis*," etc.

A. G.

2. *Recent Explorations of the Deep-sea Faunæ;* by A. E. Verrill,—A new era in the history of marine zoölogy may be said to have commenced in 1860, when Dr. Wallich obtained a number of Worms, Crustacea, Bryozoa, and Echinoderms at depths varying from 445 to 1913 fathoms, and from these observations inferred that the deep-sea had its own peculiar fauna, at depths far greater than life had previously been supposed to exist, unless in the lowest forms, such as the Rhizopods obtained from soundings and described by Bailey and others. This inference was at once confirmed by the observation of A. Milne-Edwards, who in 1861 reported a number of living mollusca and corals, found adhering to the telegraph cable between Algiers and Sardinia, when taken up for repairs, on portions that had been sunk to depths of 1093 to 1577 fathoms. Some of these were new, others were known only as Tertiary fossils. In the same year the Swedish

expedition to Spitzbergen obtained Tunicates, a Zoöphyte, a Crustacean, and a bivalve-shell from 1400 fathoms. Later, G. O. Sars has made extensive explorations by means of the dredge at the Lofoden Islands, and on the Scandinavian coast in depths of 200 to 300 fathoms, and in some cases down to 450 fathoms. Prof. Sars reports,* as the result of his son's explorations, 427 species of animals living between 200 and 450 fathoms, and representing most of the marine classes: Rhizopods 68; Sponges 5; Polyps 20; Hydroids 2; Crinoids 2; Ophiurians 14; Asterians 7; Echinoids 5; Holothurians 8; Gephyrea 6; Annelids 51; Crustacea 106; Polyzoa 35; Tunicates 4; Brachiopods 4; Conchifers 37; Gasteropods 53. He also records 9 fishes living at those depths. Of the whole number 42 were found at 450 fathoms, and 46 were confined to the region below 200 fathoms, while most of the others extend upwards to quite shallow waters. Among the many remarkable new forms the *Rhizocrinus Lofotensis* Sars is especially notable, and has been the subject of an admirable memoir by Prof. Sars.† It is remarkable as giving us a new living type of attached Crinoidea, belonging to a family most characteristic of the Jurassic formations, and is of especial interest in this connection, as it has also been found since at several other very remote localities, even as far south as Florida, at similar depths. In 1867 the Superintendent of the U. S. Coast Survey undertook an extensive exploration of the Gulf Stream, which has already yielded most important results in deep-sea zoölogy. The dredging, under the charge of L. F. de Pourtales has been carried on during three past seasons, between Florida and Cuba, from the shallow waters of the shore to the deepest waters of the mid-channel, which seldom exceeds 550 fathoms, the dredgings not having gone beyond 700 fathoms. From these explorations large and most remarkable collections have been obtained, embracing numerous representatives of nearly all classes of marine animals. A part of these have been described in the Bulletin of the Museum of Comparative Zoölogy,‡ but the Crustacea, Worms,

* Videnskabs Selskabs, Förhandlingar, 1867, pp. 246 to 275; and Annals and Magazine Nat. History, vol. 3, p. 423, June, 1869.

† See this Journal, xlviii, p. 142.

‡ No. 6.—*Contributions to the Fauna of the Gulf Stream at great depths.* By L. F. DE POURTALES, Assistant U. S. Coast Survey, December, 1867. Contains descriptions of Worms, Polyzoa, Brachiopods, Halcyonoids, Corals, Hydroids, and Crinoids

No. 7.—Same title, Dec., 1868. Contains general account of dredgings, with descriptions of Brachiopods, Crinoids, Holothurians, Corals.

No. 9.—*Preliminary Report on the Echini and Star-fishes dredged in deep water between Cuba and the Florida Reef*, by L. F. DE POURTALES; *prepared by* ALEXANDER AGASSIZ, October, 1869. Contains descriptions and tables of distribution of the Echini; descriptions of the young of Echini and their development; and a list of the star-fishes.

No. 10.—*Preliminary Report on the Ophiuridæ and Astrophytidæ dredged in deep water between Cuba and the Florida Reef*, by L. F. DE POURTALES; *prepared by* THEODORE LYMAN, November, 1869. Contains: I, General remarks and tables of distribution; II, Descriptions of new genera and species, with Critical Remarks.

part of the corals, and most of the Mollusca are still undescribed. Enough has been done, however, to reveal a wonderfully rich fauna and make us look for still greater discoveries in the future.

Another exploration,* which has yielded some of the most important results, was undertaken in 1868, by Dr. W. B. Carpenter and Dr. Wyville Thomson to explore the region between Scotland and the Faroe Islands. They were furnished with a Government vessel, fitted for the purpose, but owing to very bad weather could use the dredge only nine days during the cruise of six weeks. They obtained, however, quite a variety of animals at from 400 to 650 fathoms, including the *Rhizocrinus Lofotensis* and a remarkable variety of vitreous sponges, which seem to be very characteristic of the deep-sea fauna. But their observations upon the temperature at great depths are of the greatest importance. An extensive area was discovered between the Faroe and Orkney Islands, where the minimum temperature in 450 to 550 fathoms was from 32° to 33¾°. This region was characterized by an arctic assemblage of animals and a sandy and stony bottom. South and west from this "cold area," and extending far to the west of the Faroe Islands, was a warm area where the minimum temperature at 450 to 650 fathoms was 46° to 49°, the surface temperature being 52° to 54½°, while over the "cold area" the surface temperature was nearly the same, 50° to 52°. The warm area was characterized by a bottom of soft mud, composed chiefly of the shells of *Globigerinæ*, "*coccoliths*," "*coccospheres*" and other forms of Protozoa. Living upon this bottom were various vitreous sponges, allied to *Hyalonema*, and living like it with their long, slender siliceous spicules buried in the mud like rootlets, and several *Annelids*, *Rhizocrinus*, *Kophobelemnon*, *Terebratula*, *Lophohelia prolifera*, etc. The character of the bottom and its fauna strikingly recalled the chalk of the Cretaceous period, and the authors of these discoveries claim that *the* chalk formation has been continuous since the Cretaceous, and is *still forming* in the depths of the Atlantic. Another expedition was undertaken during the past season by the same parties, but we have seen no statement of the results, except that *Buccinum undatum* was dredged living in

No. 11.—*List of the Crinoids obtained on the coasts of Florida and Cuba, by the United States Coast Survey Gulf Stream Expeditions, in* 1867, 1868, 1869. By L. F. DE POURTALES. Contains descriptions of eight species of Crinoids (six of *Antedon*), with a table showing their distribution.

No. 12.—*List of Holothuridæ from the Deep-sea dredgings of the U. S. Coast Survey.* By L. F. DE POURTALES. Contains six species, three of which (*Echinocucumis typica*, *Cucumaria frondosa*, *Molpadia borealis*) are referred to north European species, and two others are regarded as possibly identical.

No. 13.—*Report upon Deep-sea Dredgings in the Gulf Stream, during the Third Cruise of the U. S. Steamer Bibb, addressed to* PROFESSOR BENJAMIN PEIRCE, Superintendent U. S. Coast Survey, by LOUIS AGASSIZ, November, 1869. Contains a general account of the work done and its results, together with an account of the rock formations now being deposited in that region, also observations on the young stages and mode of growth of several genera of corals, etc.

* Proceedings of the Royal Society, vol. xvii, p. 168, December, 1868; and Annals and Mag. Natural History, vol. 4, p. 112, August, 1869.

1,300 fathoms,* Dr. E. Perceval Wright also made an exploration 30 miles off Setuval, Portugal, in search of *Hyalonema*, which he dredged in 450 fathoms, and first ascertained its true mode of life.† From the same region he and Dr. Gray have described three species of Halcyonoid corals. By the same expedition a shark and a small fish were caught at the same depth. The recent Swedish expedition to Spitzbergen also made important discoveries, of which we have not yet seen full reports.‡ Dr. Smitt and Mr. Ljungman on the cruise of the Swedish frigate "Josephine," which visited the United States last summer, dredged some interesting animals on the newly discovered "Josephine Bank," between Lisbon and the Azores, in 117 fathoms; among them are *Ophiomyces frutectosus* Lyman, *Pteraster militaris*, *Rhizocrinus Lofotensis* Sars, and *Echinocucumis typica* Sars, all of which have been found also off Florida by Pourtales.§

These discoveries have very important bearings upon Geological science and Physical Geography, as well as Zoölogy, and will cause important changes in many generally accepted theories. The following are some of the results already made out:

1. It is certain that animal life does not begin to diminish sensibly at 100 fathoms and nearly disappear at 300 fathoms, as supposed by Forbes and generally believed previous to these late investigations. It is equally certain that the greatest depths yet reached with the dredge have an abundant and diversified fauna, with representatives of most classes of marine animals. The deep-sea animals are in part new and peculiar to great depths; in part found also in shallower waters; in part previously known as Tertiary or Cretaceous fossils.

2. It follows that abundance of fossils in a geological formation is not, of itself, evidence of shallow-water origin.

3. It is certain that *bright colored* animals are found at the greatest depths yet explored, and although uniform red and white are the most common colors among deep-sea animals, yet examples of nearly all the other colors have been observed, among Protozoa, Radiata, Mollusca, and Articulates,‖ as in fact we might have an-

* American Naturalist, vol. 3, p. 383, Sept., 1869. Since this article has been put in type I have had the pleasure of reading the preliminary reports of this most important expedition, by Mr. J. Gwyn Jeffreys, in "Nature," pages 135 and 166, Dec., 1869. The explorations on the Atlantic coast of Ireland by Mr. Jeffreys, extended down to the depth of 1476 fathoms, revealing a diversified fauna of an arctic character at all depths. Prof. W. Thomson conducted the explorations in the northern part of the Bay of Biscay, where the dredgings reached 2435 fathoms, with excellent results. Dr. Carpenter took charge of the additional explorations north of Scotland. The results of this expedition are of great interest and the new discoveries very numerous. It added 117 species of mollusca to the 451 previously regarded as belonging to the British fauna. Of these 56 are new to science. The mollusca obtained in 2435 fathoms are *Pecten fenestratus* (Mediterranean), *Dacrydium vitreum* (Arctic), *Scrobicularia nitida* (Finmark to Sicily), *Neæra* (new), *Dentalium* (new).

† Annals and Magazine of Nat. Hist., Oct., 1868.

‡ The Swedish expedition of 1868 obtained in soundings a *Cuma* and a fragment of *Astarte* from 2600 fathoms.

§ Bulletin Mus. Comp. Zoöl., pp. 347 and 357.

‖ See the remarks of Prof. Sars on this subject in the work quoted above.

ticipated from the fact that many burrowing annelids, crustacea, and shells, which are rarely if ever exposed to the light, are brilliantly colored.

Therefore the presence of color-markings on fossil shells is not an evidence of their shallow-water origin, as believed by Forbes and others. The finely colored specimens of *Voluta Junonia*, dredged living by Pourtales in 350 fathoms, are a sufficient refutation of this theory.

4. Prof. Sars mentions several species of deep-sea Crustacea having perfectly developed eyes, showing that light penetrates to far greater depths than is commonly supposed.* But the decrease of sea-weeds and their almost complete disappearance at about 100 fathoms may perhaps be due to the diminution or modification of the light

5. The generally received theory that the temperature of the water at great depths is everywhere the same, in all latitudes, viz: that at which sea-water has its greatest density, formerly said by Herschel and others to be 39°, but more recently shown to be that of the freezing point of sea-water (25°·4 to 27°·4 Fahr.), is not true, at least for depths down to 700 fathoms, as shown by the observations of Thomson and Carpenter.

6. The same observers have satisfactorily shown that temperature is the main agent in determining the distribution of deep-sea animals, which had been previously shown to be the case among shallow-water forms, and that mere *depth* or *pressure* has little or no influence directly. But the chemical composition of the water (as the amount of oxygen) and the character of the bottom doubtless have their influence.

7. The last observation is connected with the discovery of two contiguous, but distinct, deep-sea faunæ, living side by side at similar depths (400 to 600 fathoms); one eminently arctic in character, occupying the "cold area" on a sandy and stony bottom; the other similiar to that of the region between Florida and Cuba, and occupying the "warm area," with a bottom of calcareous infusorial mud. The last fauna, or at least some of its species, extends over a region of vast extent, both in latitude and longitude, some of the species ranging from Florida to within the Arctic Circle, and on both sides of the Atlantic.

8. The discovery of species, genera, families, and even sub-orders, supposed to have become extinct, leads us to expect that other lost forms may still have living representatives in the vast unexplored regions of the ocean.† Among the remarkable discoveries, may be mentioned the *Rhizocrinus*; the *Haplophyllia paradoxa* Pourt., a living representative of the "Rugosa" group of corals, supposed

* Other remarkable instances are mentioned by Jeffreys: as *Lacuna tenella* and a stalk-eyed crab from 808 fathoms; *Trochus minutissimus* Mighels and *Ampelisca* from 1230 fathoms; a stalk-eyed crustacean from 1476 fathoms; a large new *Fusus* from 1207 fathoms; a *Pleurotoma* from 2090 fathoms; and *Octopus cocco* from 632 fathoms, all of which are said to have well developed eyes.

† The Porcupine expedition obtained a number of species of shells previously known only from the Tertiary (Coralline Crag and Red Crag).

not to have lived since the Carboniferous period; various new forms of sea-urchins, described by A. Agassiz, having their nearest allies in the Cretaceous, etc. The discovery of the living Cystidean noticed in the last number of this Journal is another fact of the same kind, and so remarkable that it may not seem unreasonable to anticipate hereafter the discovery of living Ammonites and Trilobites.

9. These investigations have thrown great light upon the mode of deposition of certain geological formations, especially the chalk, and at the same time illustrate the manner in which, under the influence of currents of different temperatures, a chalk and a sandstone, with entirely distinct faunæ, may be deposited side by side at the same depth and, supposing the currents to be modified, how the two might alternate, thus accounting for the extinction of faunæ and the re-appearance of "colonies," as noticed by Barrande.

10. Dr. Carpenter and Thomson claim that the chalk formation now depositing in the deep Atlantic is a direct continuation of *the chalk* formation of the Cretaceous period, some of the living species being regarded as identical with Cretaceous fossils, while others, closely allied to fossil forms, are supposed to have been modified during the lapse of time by "natural selection," or in some other way.

3. *Catalogue of the Mammals of Massachusetts; with a Critical Revision of the Species;* by J. A. ALLEN. Bulletin of the Museum of Comp. Zoölogy, No. 8. Cambridge, October, 1869.—This includes not only a list of the species, with their principal synonyms, but also valuable information concerning their distribution, and their relations with the species of the old world. Mr. Allen admits much greater variations in the species of mammals than most modern writers, and shows that this variation is often as great in specimens from different parts of North America as between specimens from the two continents. Consequently he has regarded many of our mammals as identical with those of Europe and Asia. Among these are the black bear, common wolf, red fox, two weasels, mink, sable, beaver, moose, and reindeer. In some cases, however, he has probably gone altogether too far in uniting species; as in considering the black-bear, the grizzly-bear, the barren ground bear, and the European bear all one species; and in his treatment of the genus *Blarina.* He has also overlooked a specimen of *Neasorex palustris* in the Museum, from Norway, Me., described by me in the Proc. Boston Society of Natural History. V.

IV. ASTRONOMY.

1. *Report on the Recent Eclipse of the Sun;* issued under the direction of the Superintendent of the Naval Observatory.—This handsome volume of more than 200 pages has appeared first of any of the reports of the government expeditions, only detached portions of those of the Nautical Almanac and Coast Survey having yet been published. Commodore B. F. Sands,

Supt. of the Naval Observatory, took early measures to secure thorough observations, and assigned four of the five professors and two of the other astronomers at that institution, to the duty of observing the eclipse. In pursuance of the plans Prof. Hall and Mr. Rogers repaired to Plover Bay, on the Asian shore of Behring's straits, Professors Newcomb, Harkness and Eastman established themselves at Des Moines in Iowa, and Mr. Bardwell went to Bristol, Penn. The War Department lent its assistance by detailing Dr. Curtis of the Army Medical Museum, who has won deserved distinction by his success in micro-photography, and who accompanied the Des Moines party. The volume contains reports from each of these gentlemen, and to these are added others from Mr. J. H. Lane, of Washington, from Mr. W. S. Gilman, Jr., of N. Y., a lover of astronomy who has given special attention to solar observations, and from Gen. A. J. Myer, chief signal officer of the army, who witnessed the eclipse from the summit of a mountain in Southwestern Virginia,—these with the prefatory introduction of Commodore Sands composing the work now issued, which is copiously illustrated with cuts and engravings.

Prof. Newcomb's point of observation was near the Court-house in Des Moines, latitude 41° 35′ 4″, on long. $1^h\ 6^m\ 17^s{\cdot}0$ west from Washington. Following out the ideas previously suggested by him in this Journal, he affixed circular screens of different diameters to a horizontal arm, projecting from the gable end of the Court-house, so as to occult the sun for his points of observation; the largest being intended to hide the corona from the field of his 4-inch comet-seeker. After a sharp determination of the first contact with a power of 40, in this instrument, and an aperture reduced to one inch, he carefully pointed an 8-inch telescope, roughly mounted for the occasion, and the comet-seeker, to the sun at known moments; thus fixing their positions for the purpose of mapping the places of any objects which the totality might disclose in either field of view, after which he designed moving the telescopes into other positions, and counting any objects which might there be visible. Then retiring into a dark place, he awaited the total phase.

The corona appeared less bright than he had anticipated, and careful scrutiny along the ecliptic showed not the faintest trace of any flush of light extending beyond the rest in that direction. Nothing was visible in either of the two telescopes, nor did a sweep near the ecliptic with the comet-seeker disclose any object whatsoever. This important question being thus answered, and the visibility of any inferior planet or group capable of accounting for the motion of the perihelion of Mercury being made highly improbable, Prof. Newcomb proceeded to the study of the corona, with and without the aid of the comet-seeker. The great protuberance in the S.W. quadrant seemed to him strongly pink, not uniform in structure, nor at all resembling a flame, but like a huge pile of cumulus clouds, illuminated by white and red light. He was able to trace the corona with his naked eye only to a distance equal to the moon's semidiameter and he saw no long rays of light,

but a jagged and serrated edge. The sky at Des Moines was however quite hazy. But seen through a green glass the corona seemed composed of four or five prominent portions, smooth in outline, shading away imperceptibly, and in no case exceeding 5′ in height. He regards the serrated appearance and the pointed rays as subjective phenomena. The moment of end of totality was noted with the naked eye, and the last contact with the comet-seeker, as before. Comparing his observations with the computed times, he finds the contacts to have occurred later than predicted by $12^{s}\cdot5$ for the first, $10^{s}\cdot4$ for the third, and $7^{s}\cdot8$ for the last; whence he infers

Peirce's ☽ — Hansen's ☉ is too great by $5''\cdot3$,

whereas, Hansen's ☽ — LeVerrier's ☉ is too great by 2·7.

Prof. Harkness constructed a building for himself, Prof. Eastman and Dr. Curtis at another point in the city of Des Moines, and determined his position as latitude 41° 35′ $35''\cdot9$, long. $1^{h}\ 6^{m}\ 16^{s}\cdot05$ W. from Washington, by means of observations which are reproduced with great fullness of detail. At the beginning and end of the eclipse he was engaged with Dr. Curtis in taking photographs, upon which he had decided to rely for the times of first and last contact; but during the totality he devoted himself entirely to the spectroscope. With this he obtained, from a very bright part of the corona in the long beam to the southward, a continuous spectrum without absorption lines, but with a single bright line at 1497 of Kirchhoff's scale. From the great protuberance at 240° he obtained the lines C and F, that near D, and three others, two of which were *b* and the well-known third hydrogen line. The remaining one was that which he saw in the corona, and in all the protuberances without exception, and recorded as corresponding to K. 1497. It must we think have been K. 1474, which was recognized by so many other observers and which Lockyer and Young have always found in the chromosphere. The moderate dispersive power of Prof. Harkness's spectroscope would, as he states, render it difficult for him to discriminate with it between near lines.

Prof. Eastman's report contains the results of meteorological and photometric observations (which during the two days preceding the eclipse must have been far from encouraging), of observations of the contacts and of the corona. Of this and the appearances of the protuberances to him, he has given two colored engravings. In each the same outline serves for the corona; and indeed he expressly states that he "could not detect the least change in the color or position of the rays during totality." Beside these labors he assisted Prof. Harkness during the totality, by directing the spectroscope. Amid so many avocations it is surprising that Prof. Eastman could have accomplished so much, and that so well. We cannot avoid the conviction that had he been master of his own time during the total phase, he would not have failed to detect the decided fluctuations of form which the corona exhibited to more than one observer at Burlington. Like Prof. Newcomb, he found the color of the protuberances strongly red.

Dr. Curtis's report relates to the photographic operations which are described with very great minuteness. To those familiar with the exquisite photographs which he has made at the Army Med. Museum, it is needless to say that his preparations were evidently made with great forethought, and that abundant skill was displayed. A haze which veiled the sky, rendered a much larger exposure requisite than would otherwise have been given, but the photographs are of high excellence. The two taken during totality are given in engraved representations, impressions from the originals accompanying. These are very fine, rivaling those of the Burlington and Ottumwa parties. For the first, the exposure was 66 seconds; but details of the protuberances are beautifully shown, which would not have borne an exposure of one-third that length in a transparent atmosphere. Even the lunar motion seems scarcely to have interfered with the definiteness of the image.

It is to be regretted that a national enterprise carried out at the public expense should ever be made an agency for personal polemics, which indeed seem peculiarly out of place when aimed at any private citizen of the same nation, since he is theoretically one of those in whose behalf the investigation has been made. The greater part of one of the quarto pages is occupied by a note in fine print in which Dr. Gould is rather sharply assailed for his views as given on p. 435 of our last number. This is no place for any maintenance of those views, but two paragraphs deserve a word of comment.

"Dr. Gould adduces, as an additional argument in favor of his assumption, the observation that the long coronal beams appeared to him to be "variable," while the "aureole" photographed was evidently "constant" during the time of totality. This argument however loses some of its force when it is remembered that to other observers the corona appeared to the eye absolutely unchangeable both in form and position during the whole period of the total obscuration."

We are far from attributing to Dr. Curtis any intention of giving his language the discourteous significance which some might infer, otherwise no rejoinder could be possible. But we would suggest that special observations devoted to a particular point should *prima facie* outweigh general negative impressions. In the present case, if Dr. Gould's evidence be regarded as for any reason incompetent, there happens to be corroboration of his statement by other gentlemen whose attention he called to this point. And there is in addition the reasonable certainty that less than a twelvemonth will decide the question definitely, since attention will doubtless be specially directed to this point at the eclipse of Dec. next. Perhaps it might be the wisest, as it would certainly be the most considerate, course, to await such decision.

In the other passage Dr. Curtis says:—

".... the circumstance affords but another example .... of the necessity that a critic before attempting to draw scientific inferences from photographic representations should himself become something of a photographer. ..... And by a singular coincidence, evidence that Dr. Gould has not a practical acquaintance with the art would seem to be afforded in this same published letter by his total misinterpretation of another purely photographic effect, viz: the apparent encroach-

ment of the prominences upon the disk of the moon as seen in the photographs. This curious appearance, instead of being due to "specular reflection" is wholly a dark-room phenomenon, as will be explained in the text."

That Dr. Curtis's explanation is entirely adequate in most cases we readily concede. But when Ulloa in 1778 saw the brightness of the protuberances projected so far upon the lunar disk that he thought the sun was shining through a hole in the moon, there were no photographs. And had circumstances brought to Dr. Curtis's notice as many dozens of cases as have come to our knowledge, in which the great protuberance of last August was seen both by scientists and laymen, with and without optical aid, to appear as "a deep notch in the moon," we think he would have qualified his language. Dr. Gould has expressed his belief that this phenomenon is due to specular reflection from the lunar surface; others may with great reason attribute it to the influence of irradiation; but it cannot be solely a photographic effect, however such effects may coincide.

Dr. Curtis has also given the results of some interesting experiments made to determine the origin of the glow seen upon the sun, around the moon's limb, on the photographs; a glow which has been found real and not the result of contrast, in all the impressions taken last August. This he believes to be a result of diffraction, a view which we fully shared, as those present at the August session of the National Academy will remember. And by experiments involving diffractive action, he has produced artificial eclipse-photographs exhibiting the same appearance. But the recent experiments of Prof. Morton, of which Dr. Curtis must have been unaware, settled the question some time since by the production of other artificial eclipse-pictures by methods which exclude diffraction, yet manifest the crepuscule as clearly as in the original photographs. Such pictures have for a considerable time been in our possession, and Prof. Morton found the explanation of the phenomenon in a local redevelopment of the negative. This explanation seems fully to account for Dr. Curtis's results as well as for his own.

Mr. Lane's attention was given to teloscopic observation of the contacts and the corona; observations which were evidently carried out with his characteristic ability, but space is unfortunately wanting to us for a full description of his paper. Among other things, he measured the dimensions of two of the flocculent masses, or "cometoid bodies," in the protuberant aggregation at 110°; and he was able to trace the outline of the moon's eastern limb for $2^m\ 5^s$ after the end of the total obscuration.

Mr. Gilman's observations were made at Sioux City, Iowa, latitude 42° 47½′, longitude $1^h\ 16^m\ 23^s$. His memoir is illustrated by numerous interesting diagrams, among which are sketches of spots and faculæ on August 5, 6, and 7. Some seconds previous to the occurrence of totality, the great protuberance and a large portion of the corona were seen by two of his companions. This protuberance (240°) was where Mr. Gilman had seen a bright double

facula just before the first contact. Examined with a 4-inch telescope, it appeared of an orange-color, dotted with minute flakes of brilliant crimson. Another highly interesting fact is that four of the party saw an object which they confidently believed to be a star near the limits of the corona, in the neighborhood of the great protuberance. The position described was that of π *Cancri* [miswritten π *Leonis*, in *Astr. Nachr.*, lxxiv, 375] which was seen with one of the telescopes at Burlington. If this star was detected or seen by the unaided eye, no higher testimony could be brought to the keenness of the observer's vision; for its magnitude is but 5·8, being only three-tenths of a magnitude brighter than the faintest star visible to the average eye on moonless nights.

Mr. Bardwell, besides observing the times of contact, searched for inferior planets, and with the same negative results which others obtained.

Gen. Myer was at the summit of 'White Top Mountain,' near Abingdon, Va., 5,530 feet above the sea level. He saw the bright specks noted by so many as flitting across the moon's disk, and which some, erroneously, as we believe, have supposed to be meteors. With a deep-red shade-glass, "while some two digits of the sun were still uneclipsed," he noted a luminous prominence of yellowish color upon the moon's limb. Many of the details of the total phase were distinctly visible to the naked eye in this clear atmosphere, and when the last line of sunlight separated into beads, the guides exclaimed that the sun was breaking to pieces!

We have dwelt so long upon the observations within the territory of the United States, that no opportunity remains to give details regarding the observations of Prof. Hall at Behring's Straits (latitude 64° 20′, longitude 6^h 25^m). After their long voyage and hopeful endurance, they had the disappointment of seeing clouds drift over the sky, about an hour before the eclipse began. Within an hour after it had ended, the sky was cloudless again. The sun and moon were invisible from a short time before until a short time after totality, but the darkness was greatest between 9 17½ and 9 20 A. M.,—during which period there was less light than at the midnight preceding. The report is accompanied by copious meteorological and magnetic observations, which bear witness to the fullness of the preparations made, and testify that nothing within the control of Prof. Hall was wanting to render his results exceedingly valuable. Capt. S. R. Franklin of the United States Steamer Mohican, saw three protuberances upon the moon's disc, but no more. B. A. G.

2. *On the flight of a remarkable meteorite across the Western portion of Ohio near Forest*, lat. 40° 50′ and long. W., 84° 40′; by J. Lawrence Smith, Louisville Ky.—A few minutes before three o'clock on the morning of Oct. 27th, the citizens of Forest, and for miles around, were suddenly aroused by a terrific sound in the upper regions like the report of a heavy siege gun, followed by two or three reports in quick succession, resembling the reports of field pieces fired by section, and ended in a peculiar and rather

metallic, rumbling or roaring sound, dying away in the distance. The interval between the first and successive reports was two or three seconds; the other intervals about one second. The firmest houses were shaken, windows rattled, and thousands of sleepers were roused in an instant, bewildered at the unusual and appaling noise; and many were the conjectures as to what it was; some thought it was an earthquake, but to the greater part it was simply an inexplicable phenomenon. Persons who happened to be awake at the time, were first startled to see the night suddenly lighted into day, and soon relapsed again into the usual darkness. While wondering what caused it, the stunning sound broke in upon them and greatly increased the mystery. The time between the going out of the light and the report is estimated by citizens here at from one-half to one minute.

Mr. Pierson of Patterson, a village about one mile, a little west of south of Forest, states that he saw the meteor coming directly toward him, from a direction about S. 35° W.; that it was a ball of fire apparently as large as a bucket, exceedingly bright and dazzling and had a luminous tail apparently thirty feet long and three feet wide; that it vanished or exploded, as he thought, directly overhead. At Finly, twenty miles northwest of Forest, the statement is, that there at about 3 o'clock on Wednesday morning, Oct. 28th, the inhabitants were aroused by a terrific explosion somewhere in the upper regions.

The night was one of clear moonlight, and exceedingly cold for the season. The night watchmen had witnessed it; and one says the he first saw it in the southeast, in size, seemingly, as large as a beer keg, and of intense brightness; that it descended, leaving a luminous streak behind, and that when near the earth, it exploded, with a terrific sound, and fierce brightness; that the light, after the explosion, took a southerly course, and disappeared. Another watchman reports that at the time of the explosion, it appeared as large as a load of hay, and that the sound of the explosion was stunning, not like a quick sharp report of thunder, but, as he termed it, more like the coming together of railroad cars, but much louder, and that the light was brighter than that of the sun. The direction of the meteor from Finley, as given by the watchman, with the bearing of the meteor's path, as described by Mr. Pierson of Patterson, and the fact, that to many the sound seemed nearly overhead, would indicate that it exploded or terminated its course in the vicinity of Forest; yet a careful investigation might prove its terminus to be many miles from that place. The sound seems to have been heard for perhaps fifty miles around, if not more. The stones or fragments that have fallen, may never be found, owing to the fact that the explosion was at night, and the consequent difficulty of determining its exact locality. In Kenton, Ohio, the phenomena are said to have occurred a few minutes before 3 o'clock and consequently they were not well observed; many persons saw the light but not the meteor, and all were sensible of the shock and sound. The meteor did not pass

this place nearer than twenty miles, and the best judges give its duration at from two to three minutes from the flash to the explosion. The sound was of such force as to shake the houses and many believed it to be an earthquake.

These are all the statements I have been able to obtain in regard to the appearance of this meteor and its accompanying phenomena. It was beyond all doubt a meteorite, and I am using all possible means to discover any fragments that may have fallen. I must acknowledge the assistance afforded by Mr. Moore and Mr. Thomson, of Ohio.

3. *Elements of Asteroid* (109); by Wm. A. Rogers. (From a letter dated Alfred Observatory, Alfred, N. Y., Dec. 21st, 1869.)—I herewith communicate for the Journal the elements of (109) which I have computed from the following observations:

Date, W. M. T.				Place of observation.	α			δ		
	h.	m.	s.		°	′	″	°	′	″
Oct. 9,	13	26	32,	Hamilton Coll. Obs.,	14	00	45·9	+9	37	15·0.
Oct. 31,	8	44	33,	Hamilton Coll. Obs.,	9	11	27·8	9	54	47·8.
Oct. 31,	8	44	33,	Alfred Observatory,	9	11	32·4	9	54	48·0.
Nov. 28,	7	15	57,	Alfred Observatory,	8	7	49·2	11	9	3·17.

Epoch Oct. 9·0, W. M. T.

Mean Eq. 1869·0

$$M = 339^\circ\ 16'\ 02''\cdot0$$
$$\pi = 55\ 22\ 43\cdot3$$
$$\Omega = 4\ 57\ 35\cdot2$$
$$i = 8\ 4\ 10\cdot6$$
$$\varphi = 17\ 16\ 40\cdot7$$
$$\mu = 804''\cdot8304$$
$$\log a = \cdot4295348$$

For computing an ephemeris, I find:

$$\log x = 9\cdot9999681 + \log \sin (v + 145^\circ\ 19'\ 47''\cdot4)$$
$$\log y = 9\cdot9307966 + \log \sin (v + 55\ 45\ 21\cdot1)$$
$$\log z = 9\cdot7181204 + \log \sin (v + 54\ 11\ 42\cdot2)$$

V. MISCELLANEOUS BIBLIOGRAPHY.

1. *Exercises in Practical Chemistry*, by A. G. Vernon Harcourt, M.A., F.R.S., Sec. C. S., and H. G. Madan, M.A., F.C.S. Series 1st, Qualitative Exercises. 335 pp. 12mo, Oxford, at the Clarendon Press, 1869. London, Macmillan & Co.—This little volume is designed as the beginner's *vade mecum* in commencing the study of practical chemistry in the laboratory. No attempt at a systematic presentation of the elements of the science is here made. The novitiate is presumed to be equally innocent of nomenclature, symbols and philosophy and is led into the laboratory much as an apprentice to a trade, and is therefore first made familiar with his tools, and how to use them in the most simple operations before even attempting the preparation of oxygen and other gases. It is illustrated by sixty-five wood cuts, mostly new and many of them very effective. The nomenclature is that of Williamson. Symbols

expressing the more important reactions are given at the foot of the pages where needed. We look with interest for the second series on quantative chemistry.

2. *On Aniline and its derivatives. A Treatise upon the manufacture of Aniline and Analine Colors*, by M. REIMANN, P.D., L.A.M., to which is added, *The report on the coloring matters derived from coal tar, shown at the French Exhibition*, 1867; by Dr. A. W. HOFMANN, F.R.S., MM. G. de LAIRE, and CH. GIRARD. The whole revised and edited by WILLIAM CROOKES, F.R.S. &c. 8vo, pp. 164. (John Wiley and Son, Astor Place, New York, 1868.—Dr. Reimann's account of aniline and its derivatives is a fine example of the union of exact science, with practical skill, and as such teaches an important lesson beyond its special theme. It could not have had a more valuable supplement than in the admirable report of Dr. Hofmann and his associates, on the coal tar colors shown at the French Exhitition of 1867. The book, though published by Messrs. Wiley, was printed in London, by Mr. Crookes at the office of the Chemical News.

3. *A short Course in Qualitative Analysis, with the New Notation;* by J. M. CRAFTS, Prof. of General Chemistry in the Cornell University. 133 pp. 12mo, with five Tables. N. Y., 1869. (J. Wiley & Son.)—Prof. Crafts is well known to our readers from his frequent valued chemical contributions. He has, in the small volume before us, attempted the solution of a problem which every chemical instructor must meet whose duties call him to impart to a mixed class of academic students a maximum of knowledge in a minimum of time. A considerable portion of the first two chapters is devoted to an explanation of the theory of chemical reactions and nomenclature. The student is at once inducted into the notions of modern chemistry and familiarized with atomicity and the present chemical nomenclature. It is certain that under a good teacher any faithful student will master the main points of qualitative analysis by the time he has gone through the second part of this useful little volume. Only 34 of the 64 radicals known to chemists are treated of in this book. This brevity sometimes mars symmetry and renders the work of the student almost too simple, as when, for example, strontium is left out of group II. The tables IV and V, intended to record in a compact form the facts of analytical chemistry, are ingeniously devised by Mr. Perkins, to give the student exact ideas and methodical habits.

4. *The Fruits and Fruit trees of America, &c.;* by A. J. DOWNING. *Second revision and correction, with large additions*, by CHARLES DOWNING. pp. 1098, 8vo. New York, 1869. (John Wiley & Son).—Those cultivators of fruits who have been accustomed to handle the small duodecimo volume, which was left us by its lamented author under the above title, will hardly recognize their old acquaintance now grown to such portly dimensions. But the same rural flavor and discrimination are found in the work which Mr. Charles Downing has given us, that rendered the original edition of this work by his gifted brother a universal favorite on both sides of the Atlantic. But under Mr. Charles Downing's editorship

this work has become in fact a pomological encyclopedia, embracing a vast number of varieties of various fruits, which are of interest to the curious reader rather than the cultivator. For example, not less than 2800 varieties and synonyms of the pear are mentioned, while few cultivators in the Eastern United States plant over twenty or thirty sorts. Fortunately for the novice in pomology, Mr. Downing adds well considered select lists of the various fruits, that of the pear containing about seventy varieties, which serve to redeem the planter from chaos. The chapter on the grape, is enriched by well drawn figures of the most approved sorts of American origin, most of which have been produced by cultivation since 1845, when the first edition of Downing was published. It would have been well if the general introduction to this fruit had been rewritten or extended to embrace important new matters, as the sulphur treatment for *Oidium Tuckeii*, the later researches on the insects infecting the vine, and the considerable experiences which twenty years have given us, relating to vineyard culture and American vines.

The title of this book reminds us that its contents do not correspond to what it calls for. To one familiar with fruit culture as it exists on the Pacific coast of the United States, Mr. Downing's book appears more foreign than any European work on horticulture can to a cultivator living upon the Atlantic shores of the continent. It would have given a completeness to the work, if it had briefly sketched the main peculiarities of the most remarkable fruit-growing region of North America.

5. *Agricultural Qualitative and Quantitative Chemical Analysis*, (after E. Wolff, Fresenius, Krocker and others), by G. C. Caldwell, Professor of Agricultural Chemistry in the Cornell University. 300 pp. New York; (Orange Judd & Co.).—This work, prepared from the best sources in a thoroughly conscientious and judicious manner, supplies a want now beginning to be seriously felt in this country.

Prof. Caldwell's book is intended to serve as a complete manual of chemical analysis for the use of agricultural students. The titles of its chapters are—I, Reagents. II, Analytical Operations. III, Reactions and Methods of Quantitative Estimations. IV, Special methods of Analysis, viz: Course in Qualitative Analysis, including the elements encountered in Soils, Plants and Agricultural Products, Estimation of Water, Organic Matter, Sulphur and Chlorine in Organic Compounds, Separation and Estimation of the Alkalies, Alkali-Earths, Alumina, oxyds of iron and manganese, silica and phosphoric acid. V, Analysis of Soils, Rocks and Marls. VI, Analysis and Valuation of Fertilizers. VII, Ash-Analysis. VIII, Analysis of Fodder and Food. IX, Wool and Bark. X, Beverages. XI, Tables. We trust that this volume will be studied and used by every student in our Agricultural Colleges, for the knowledge that can be acquired only by following and by applying its methods is not only of the utmost importance to the individual farmer, but bears most seriously upon the devel-

opment and conservation of our greatest national resource, the productive power of our soil. J.

6. *Lithologie des Mers de l'ancien monde, par* M. DELESSE.—We have received from the author two beautiful charts, illustrating the above work, which we propose to notice in another number.

7. *Hydraulic Motors;* translated from the French cours de Mécanique appliqué, par M. BRESSE, Professeur de Mécanique a l'Ecole des Pronts et Chaussées, by F. A. MAHAN, Lieutenant U. S. Corps of Engineers. Revised by D. H. Mahan, LL.D., Professor of Civil Engineering, &c., United States Military Academy. 165 pp. 8vo. 1869. New York, (John Wiley & Son.)—A convenient and thorough treatise on the subject of Hydraulic Motors.

8. *Weisbach's Manual of Mechanics*, vol. i, part II, 8vo. New York, (D. Van Nostrand).—The second part of the first volume of Weisbach's Mechanics, noticed in our last volume (p. 449), has just been received.

9. *A Practical Treatise on Metallurgy, adopted from the last German Edition of Kerl's Metallurgy*; by WILLIAM CROOKES, F.R.S., and ERNST RÖHRIG, Ph.D., M.E. In three large volumes, 8vo. London, (Longmans & Co.; New York, John Wiley & Son, 2 Clinton Hall, Astor Place.)—These volumes take up the subjects in the following order: Vol. I, Lead, Silver, Zinc, Cadmium, Tin, Mercury, Bismuth, Antimony, Nickel, Arsenic, Gold, Platinum, Sulphur. Illustrated by 207 wood engravings. 724, pp. 1868. Vol. II, Copper, Iron. Illustrated with 273 wood engravings. pp. 876, 1869. Vol. III, Steel, Fuel; Supplement. Illustrated with 145 woodcuts. 803, pp. 1870. We have no space at this time for a further notice.

10. *Reliquiæ Aquitanicæ;* by Messrs. LARTET & CHRISTY, edited by Prof. Thomas Rupert Jones. Part IX, of this beautiful work on the Archæology of Southern France, has been published.

Lea on the Genus Unio. Index to Vol. XII and supplementary index to Vols. I to XI of Observations on the Genus Unio, together with Description of new species of the Family Unionidæ and description of new species of Melanidæ, Paludinæ, Helicidæ, etc. Isaac Lea, LL.D., etc. Philadelphia, 1869.

Bulletin of the National Association of Wool Manufacturers. Oct., 1869. Boston.

Address delivered on the Centennial Anniversary of the birth of Alexander von Humboldt under the auspices of the Boston Society of Natural History, by Louis Agassiz. Boston, 1869. 108 pp. 8vo.

Mammalia of Massachusetts, by J. A. Allen. 112 pp. 8vo. Bulletin of the Museum of Comparative Zoology at Harvard College, Cambridge, Mass. No. 8. 1869.

ALEXANDER VON HUMBOLDT, eine wissenchaftliche Biographie, by Dr. Carl Bruhns, aided by various Savans of Germany, is soon to be issued in two large volumes by F. A. Brockhaus of Leipzig.

OBITUARY.—THOMAS GRAHAM, author of the excellent "Elements of Chemistry," and since 1855 Master of the Mint, died Sept. 16th, in his 64th year.

AXEL JOACHIM ERDMANN, Director of the Geological Survey of Sweden, and eminent in Mineralogy as well as Geology, died at Stockholm on the 1st of December last, at the age of 55 years.

O. L. ERDMANN, the distinguished Chemist, and editor of the Journal für praktische Chemie since 1843, died on the 9th of October at Leipzig, aged 65 years.

MICHAEL SARS, the eminent zoologist of Norway, and one of the first in Europe, died at Christiania on the 22nd of October last, at the age of 65 years. The last of his remarkable memoirs is noticed in the last volume of this Journal, (p. 142).